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The Inaccuracy of Movement

WITH SPECIAL REFERENCE TO CONSTANT ERRORS

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THE INACCURACY OF MOVEMENT

INTRODUCTORY

The student of the psychology of movement is, to say the least, not hampered by the novelty of his subject. Ever since the days of the muscle sense controversy investigator after investigator has interested himself in the subject of movement until a considerable body of motor psychology, in turn acclaimed and condemned, has developed. The present study is not directly concerned with the implications of movement, nor with the relation of movement to mental processes with which an analytic psychology is largely concerned. As an experimental investigation it grew out of a number of interesting and not at once explicable observations of constant errors in exercises on the accuracy of the perception and reproduction of arm movements. The accuracy of movement has frequently been the subject of special study, from different points of view and not infrequently with varying or inconsistent results. As has often been pointed out, this inconsistency is partly due to the extreme complexity of the sensations aroused by the movement of the parts of the body usually employed—chiefly the upper and lower limbs and the eyes. Introspective analysis of the sensation of movement is exceedingly difficult. Coming, as it does, from a great number of sources—the muscles, ligaments, tendons, articular surfaces and skin—and closely associated as it is with the spatial order of other senses, particularly that of vision, it seems to present a highly complex fusion, the components of which do not readily yield themselves to the efforts of introspective discrimination.

Further, as will be more fully developed in a later chapter, the process of recognition and judgment of extent seems to consist, first, of a reference of the movement to a familiar and rather loosely defined group, followed by its approximate recognition as this or that movement. Many of the constant errors and illusions of movement may be found to depend on the nature of this process. A third source of discrepancy in comparative results is shown by the present experiments to lie in the methods of control and record used. The present experiments seem to indicate that every movement as judged tends to fall into its proper place in the objective scale of magnitude as determined on some other basis than the intensity or extensity of the accompanying sensations, or of the stimulus, though not entirely

without reference to these factors. Investigation of the relations and interdependencies of these objective characteristics and of the influence any one of them may exert on the judgment of any other are not without interest. Not a few such researches have been conducted, but there are still questions that have not been satisfactorily answered, sources of error that have not been sufficiently regarded, contradictory results that have not yet been cleared up, and interesting phenomena which seem to have escaped observation. Theories as to the more fundamental character of judgments of time or of extent have not been tested in any direct way, illusions of over and under estimation have not been satisfactorily accounted for, statements of the "law of forgetting" for spatial and temporal magnitudes can not yet be generalized, no convenient apparatus for recording simultaneously the extent, speed, duration and force of movements of any considerable magnitude has yet been described.

The experiments to be reported in the following chapters were conducted with the following chief things in mind:

1. The desire to find the most satisfactory method of recording and studying voluntary movements of the limbs, and to construct a simple apparatus which would measure, simultaneously, all the attributes of any single movement.

2. The determination of the relations existing between the perceptions of extent and of duration or speed.

3. The analysis of the basis of the positive and negative constant errors involved in the phenomenon of the shifting or periodic "indifference point."

4. Investigation of the extraordinary positive constant errors produced by the force of impact.

5. The testing of the "duration" hypothesis in the constant errors of the Loeb illusion.

6. The need for further data on the memorability of spatial and temporal perceptions.

7. The unsatisfactoriness of most expositions of the basis of the judgment of equality or difference in the case of space magnitudes.

The experiments were performed in the Psychological Laboratory of Columbia University during the years 1907–9, under the directions of Professors Cattell and Woodworth. The writer wishes also to express his obligation to Professor T. L. Bolton, of the University of Nebraska, under whom he received his first scientific training and to whom he owes his first interest in the psychology of movement.

CHAPTER I

Methods of Studying Movement

In this chapter will be considered some of the methods employed by various other investigators in this field, and a piece of apparatus especially constructed for the present research will be described—an instrument which seems in many ways to possess advantages over the apparatus heretofore used. In the succeeding chapters will be reported the experimental results of the investigations undertaken, with a discussion of their significance for the general problem of movement.

(a) Extent of Movement

The study of method, always important to the success of an experimental investigation, has special significance in the psychology of movement. The divergent results of different investigators in this field seem more frequently to indicate the peculiar influence of the method employed than to display the character of the perception of movement as such. The first and most comprehensive study of method in the field of movement, that of Cattell and Fullerton,[1] was concerned chiefly with the investigation of the reliability and influence of the various psycho-physical methods, the mode of judgment and of record. These authors conclude that "the method of average error—in which the observer makes one stimulus as nearly as possible like another—is in many cases the most convenient of methods,"[2] and this method of average error was employed throughout the present study. But the chief divergence between the results of different authors seems to have been caused by differences in the objective, instrumental methods employed, and these methods require more thorough investigation before many results can be completely interpreted.

The controversy over the question of rectilinear and curvilinear movement seems to be still undecided. Külpe[3] and Angier[4] insist that all the work on rectilinear movement is worthless, and that the whole matter must be worked over, using movements of the curvilinear type—rotations of a single joint in a single plane, while Woodworth[5] has clearly pointed out that "the force of this objection is

[1] "On the Perception of Small Differences," 1892.

[2] *Ibid.*, 151.

[3] "Outlines," 341.

[4] *Zeit. f. Psychol.*, **39**, 430, 1905.

[5] "Le Mouvement," Paris, Doin, 1903, 89.

more apparent than real." The matter need not concern us here, since we are not interested in the statement of the absolute quantitative accuracy of either the perception or the reproduction of movements, nor in a determination of individual differences, nor in any attempt to give topographical location to the source of the sensations of movement. In only one case, that of the experiments on the degree of contraction, would the type of movement used suggest any possible source of error for the problem set. But since other experimenters have shown the Loeb illusion to occur with curvilinear as well as with rectilinear movement, even here the type of movement is irrelevant to the topic under consideration.

The methods of active and passive movement have also frequently led to somewhat different results, but it has been generally recognized that the two situations are qualitatively different, and this disparity of method has seldom led to erroneous interpretation. In fact this has happened only in cases in which the block method, next to be discussed, was employed in the active movements and the peculiar error characteristic of this method allowed to pass unexamined.

The traditional method of controlling the extent of a movement to be judged is by impact of the moving member or the carriage against an upright. The possibility of error in the use of this method has already been suggested incidentally by Titchener[6] and by Segsworth.[7] The latter regrets that no other method is possible unless shadows, photography or some other such optical apparatus be employed. The objections made to the method are that groups of other sensations, consisting of contact, pressure and resistance, are brought about, and "complicate the judging of the pure motion sensations."

The present study will show[8] that this method introduces a large positive constant error, which is a function, in part, of the force of impact against the block, and the magnitude of which causes a corresponding increase in the variable error. The only other principal method which seems to have been previously used is the "free" method, by which the subject makes a movement which is self-controlled as to extent and time. Then having made this free and predetermined movement, another movement is made which is to be equal to the first. The faultiness of this method is apparent. What the subject tends to do is to endeavor to make two movements according to a mental standard, which may even be so standardized as to be expressed in inches or millimeters. It is not, in this case, a matter

[6] "Exper. Psychol.," Vol. II., Pt. 2, 260.

[7] *Amer. Jour. of Psychol.*, 6, 369, 1894.

[8] Chapter II.

of the reproduction of a previous movement. Indeed the perception of extent hardly enters except as the subject is required, after having made the two movements, to indicate their relative magnitude—to guess at his probable error. In this case it is at all events hard to secure well-distributed and uniform records.

From still another point of view there are two methods, both of which have been used by different investigators. These may be called the continuous and the successive methods. With the continuous method, the starting point of the second movement coincides with the terminal point of the first one. The two movements are thus not only made with a different degree of contraction of the muscle, but in some cases different or additional muscles are brought into play. This would of course be no objection from the point of view of one who adheres to the joint sense theory. But as a matter of fact a constant error is here introduced, the nature of which will be pointed out in the next chapter. The other method also presents difficulties. Kramer and Moskiewicz claim that in reproducing from an identical starting point, a tendency to grope for the same terminal position results, and the feeling of movement reduces to a feeling of position. This tendency is clearly present in the case of some subjects. But it seems that one method or the other must be used, for no other practical alternative has yet been suggested.

The apparatus later to be described is designed to eliminate many of the errors arising from this diversity of methods.

(*b*) Time of Movement

Aside from reaction experiments, fewer studies have been made of the time of movement than of its extent. This has been chiefly on account of the difficulty of conveniently recording and controlling the time of movements of any considerable magnitude. Intrinsically the subject is of great interest. By means of the instrument devised for the present study the duration of a movement, its speed at any point in its course, as well as its extent, are graphically recorded. Moreover the movement may be as much as a meter in length, although extremely small movements are recorded with equal accuracy.

It may be well to point out the methods heretofore employed for the registration of this type of movement. The first investigators were Camerer[9] and Vierordt.[10] The subject was required to rest his fingers on the top of a brass rod, which was hinged at one end. The other end bore a writing point which recorded the movement on a horizontal rotating drum. This drum was turned by hand and the

[9] "Versuche üb. d. zeitl. Verlauf d. Willensbewegung," Diss., Tübingen, 1866.

[10] "Zeitsinn," Tübingen, 1868, 33.

time of the movement calculated on the basis of a time line afforded by an induction apparatus. With this method the extent of the movements studied was limited to a very few millimeters. Binet and Courtier[11] worked with rather limited writing movements, using an Edison electric pen. The needle of this pen, actuated by an eccentric at the rate of about 11,000 times per minute, pricked holes in a roll of paper. Interesting results were suggested, but on account of mechanical difficulties nothing very definite could be stated. The rate of 11,000 punctures per minute was too rapid to allow any accurate calculation. And if the rate was lowered the needle caught in the perforations, tearing the paper and interfering with its own regularity. Leuba,[12] in an unpublished study, describes an instrument to be carried by the index finger, the slightest movement of which makes a contact which is broken by lifting the finger into the air. The time is recorded on a kymograph drum. No experimental results have yet been reported, but since the mechanism involves a reaction time at the termination of the movement it seems probable that the natural course and character of the movement would be disturbed by this additional feature.[13] Jaensch[14] worked with a hollow pen holder containing a spring connected with a Marey tambour. The pen was pressed down by the subject at the beginning and end of each movement, thus recording the time of the movement on the drum by jerks in the line but necessitating a reaction time and a distracting performance at each end of the movement. Cattell and Fullerton[15] have described the instrument at present used in the Columbia laboratory in connection with a Hipp chronoscope. The beginning of a movement closes a circuit which is broken at the completion, thus registering the time from the beginning of the movement to the moment at which the circuit is broken. The chief difficulty in the use of this instrument and of the Witmer modification of it employed by Gault[16] will be pointed out in Chapter V.

(*c*) Force of Movement

In most of the work done on this subject—so far as I have been able to learn, in all except the work of Cattell and Fullerton[17]—the

[11] *Revue Philosophique*, **35**, 664, 1893.

[12] Fifteenth Report, Am. Psychol. Ass., 1906, 218.

[13] Leuba has since exhibited, before the American Psychological Association, December 20, 1908, a device for recording independently the extent and duration of forearm movements.

[14] *Zeit. f. Psychol.*, **41**, 257, 1907.

[15] *Op. cit.*, 103.

[16] *Am. Jour. of Psychol.*, **16**, 357, 1905.

[17] *Op. cit.*, 66.

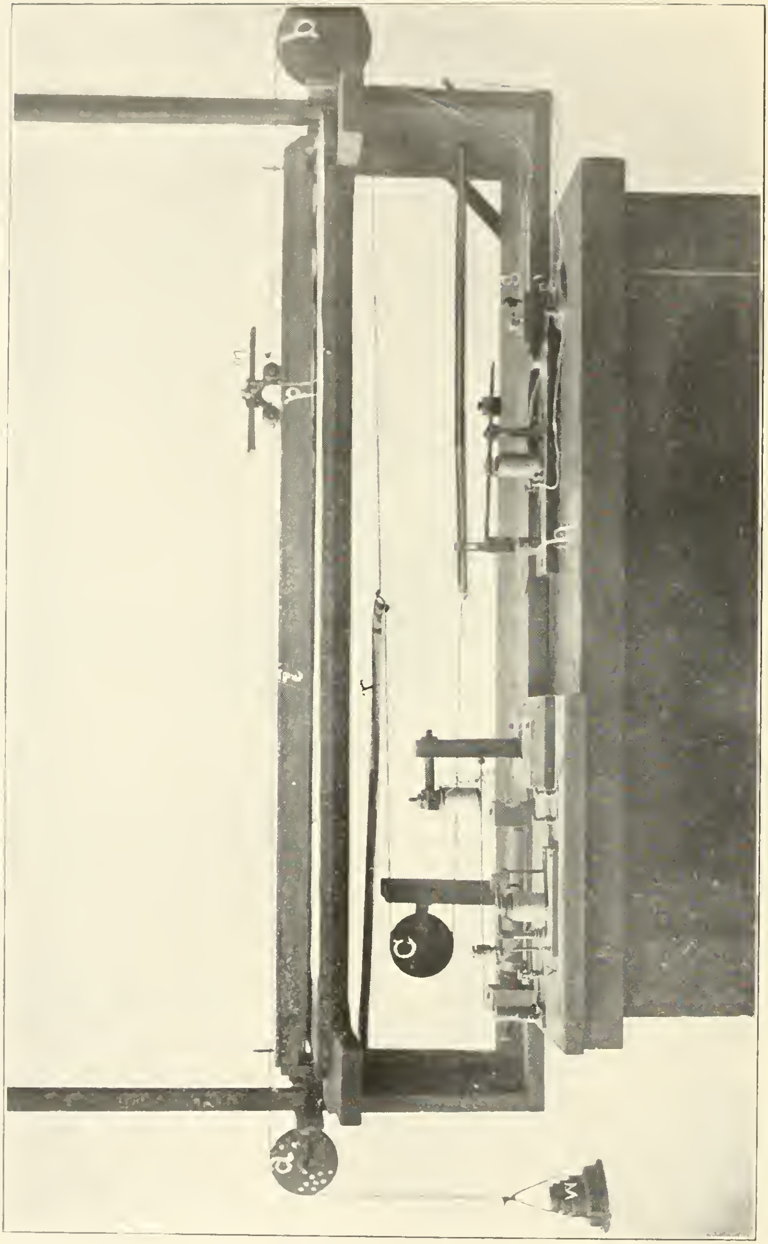

Plate I

force of movement has been a direct function of the extent. This is especially true of the vast amount of work that has been done on the ergograph. Under these conditions it has been impossible to say how far the judgment was concerned with the pure force or energy of the movement and how far it had to do with the perception of extent as a secondary criterion. Consequently in constructing the instrument about to be described provision was made for the study of the force of movement under conditions which allow the perception of force to be made independently of the perception of extent.

(*d*) The Apparatus and Method of the Present Study

As a foundation for the instrument the Cattell-Fullerton apparatus for the study of extent of movement was used. This has already been described by these authors as consisting of "a brass plate one meter long, graduated to millimeters and grooved for the wheels of a small brass carriage (see Plate 2). Along the scale is a wire, carrying an indicator (*i*) which is moved by a bar (*b*) attached to the carriage. Between the front and back wheels of the carriage, and parallel with the track, is a ring (*t*) into which is inserted the finger used in moving the carriage. . . . The carriage may be moved alone or used to raise any weight (*w*) attached to the cord."[18] For the purposes of the present experiments a number of modifications have been made. In order to more completely eliminate the noise made by the moving carriage, wood-fiber wheels have been substituted in place of the original metal ones. When a little machinist's oil is placed in the grooves of the track, the car now runs smoothly and noiselessly. In place of the original uprights for controlling the extent of the standard movements, a sound hammer (*h*, Plate 1) is arranged in circuit with the car and the indicator (*i*) which slides on the wire. The contact made by the bar running out from the carriage and the brass indicator completes an independent electric circuit which runs through the hammer magnet. The stroke of the hammer serves as a signal for the stopping of the movement. The constant error of impact, later to be pointed out, is thus avoided, and whatever reaction time is involved is included in the original time and extent of the movement.

For recording the duration of movements the following device is employed. To the top of the carriage is attached a signal magnet (*m*) which controls the vibrations of an enlarged Pfeil time marker (*s*). The magnet circuit is interrupted by means of a reed oscillator (*v*), vibrating at the rate of ten times per second. This gives ten main vibrations of the time-marker per second (c^1). But in order

[18] *Op. cit.*, 35.

to make interpolation easier and more accurate a thin piece of rubber is glued on the face of the magnet core (m). This produces a rebound (c^2) of the spring of the time-marker in the middle of each main vibration. The tenths-of-a-second curve, produced, when the carriage is moved, by the simple vibrations, is thus transformed into a twentieth-of-a-second curve, each tenth being represented by a large deflection of the tracing point, and each intervening twentieth by a somewhat smaller deflection. By interpolating within these twentieths, the time of a movement can be determined to within one hundredth of a second.

The writing point (p) of the time marker consists of a thin brass extension terminating in a piece of flexible gelatine. The record is made on a smoked paper (x) stretched on a horizontal frame which slides underneath the track from the side of the apparatus on which the operator stands. This frame is made of well seasoned wood, and is prevented from warping by means of a thin steel lining running along both sides. Each side and end of the frame consists of four layers—first a layer of wood, then in turn a layer of cork, another layer of wood, and finally the steel lining. Over the frame is tacked a foundation of cardboard, which serves to support the glazed paper while it is being attached and smoked. The paper is stretched out over the frame and fixed in place by strips of cardboard. Thumb tacks through the cardboard and into the cork layer of the frame are easily inserted or removed. After the paper is thus fixed in place on the frame the smoking is easily accomplished by moving the inverted frame above the camphor flame.

For studying the perception and reproduction of the force of movement, the carriage is made to pull against a pair of coiled springs (r, r'), placed below the box which supports the track at the proper elevation for making convenient movements of the carriage. These springs are so adjustable that the force may be varied independently of the extent, but may be correlated with it empirically, and in a relation unknown to the subject. Thus the first or standard movement may be made against one spring at a given degree of tension, while the second movement may be made against a different spring, against the same spring, or against both. A pulley attachment (a) provides for the use of weights instead of springs if such an experiment is desired.

We may thus secure, simultaneously, a graphic record of the duration, speed, extent, and force of a given movement, along with an indication of any irregularities that may occur in its performance. The method of procedure is simple enough. By closing a convenient key (s, Plate 2) on the table before him the operator sets the time

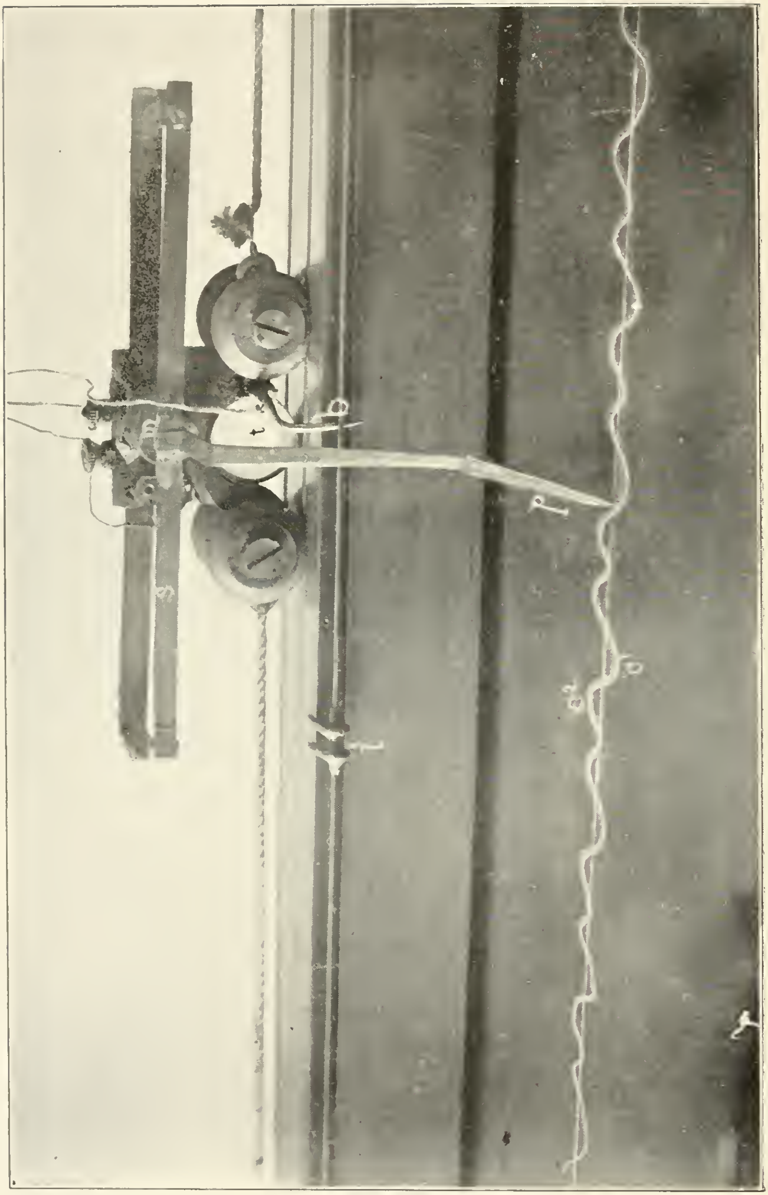

Plate II

marker in vibration. At a signal from the operator or at an independently chosen moment, the subject begins his movement. At the sound of the hammer signal he stops the movement, and the hammer circuit is broken by the operator by throwing another switch (s') near at hand. Before the carriage is returned to the starting point the magnet circuit is also broken. During the movement the writing point has traced the compound time-curve on the paper. As the carriage is returned, the writing point traces a straight line which divides the previously inscribed record in such a way that the tenths of a second may be read off on one side of the line. For the rebounds of the Pfeil spring, when the current is off, come beyond the straight line, registering tenths of seconds. But the rebounds when the circuit is closed are of smaller amplitude, and come only to the straight line without crossing it. These vibrations are ignored when counting in tenths, but when counting in twentieths, both the vibrations (c') reaching beyond the line and those extending only to it (c^2) are regarded.

CHAPTER II

The Illusion Produced by Impact

In a series of experiments performed previous to those report[illegible] in this series, the traditional method of controlling the extent [illegible] movement to be judged by blocking it by means of an upright [illegible] employed. It was soon observed that the impact of the m[illegible] against the upright produced a large constant error in the [illegible] tion of the movement. This constant error was alwa[illegible] was frequently so great as to astonish the operat[illegible] to suspect that the observer was not paying the le[illegible]on to the experiment. But careful observations of ha[illegible] dozen subjects showed that the illusion was present in all cases and experiments were made to test the direction, amount and persistence of the error.

In Table I., for Observer Lk., typical results are shown for free and blocked movements. In the case of the free movements the standard was in each case a spontaneous movement the extent of which was determined by the subject. A standard movement was made and then this standard reproduced as nearly as possible. The standard movements were deliberately varied between 75 and 350 mm. The error in per cent. was then calculated for each movement, and movements between 75 mm. and 125 mm. grouped under column 100 mm., movements between 125 mm. and 175 mm. under column 150 mm., etc. Thus, in Table I., the first column under each heading (100, 150, etc.) shows the average per cent. error for movements ranging around the magnitude indicated by the heading as central tendency, and not deviating from this magnitude by more than

TABLE I

Free and Blocked Standards

Observer Lk.

		100		150		200		250		300	
		%	mm.	%	mm.	%	mm.	%	mm.	%	mm.
Free.	A.E.	23	23	14	21	14	28	10	25	9	[illegible]
	C.E.	+ 18	+ 10	+ 10	+15	+11	+ 22	+ 7	+18	— 4	—
	V.E.	*12*	*12*	*13*	*20*	*10*	*20*	*12*	*30*	*8*	
Blocked.	A.E.	155	155	110	165	60	120	30	75	15	15
	C.E.	+155	+155	+110	+165	+60	+120	+30	+75	+15	+44
	V.E.	*28*	*28*	*27*	*41*	*15*	*30*	*13*	*33*	*9*	*27*

25 mm. The second column gives the error in mm., found by multiplying the central tendency by the average error in per cent. In the ecorded in the lower part of Table I. the subject was simply d to move along the track until his movement was blocked. The upright was then removed and the movement was continued, in an endeavor to make the two extents equal. The table gives the gross average error, the constant error and the variable error, in mm. and in per cents of the standard, fifty trials being made each magnitude under each type of movement. In both cases the ious" method was used—the terminal point of the first moveing as the starting point of the second. Any other method interfere with the illusion. Thus if the car had been initial position and the second movement made over the sam of track, the illusion produced by the impact would tend to be partly corrected by the more careful and precalculated movement back to the starting point.

The upper half of Table I. gives the records for the free movements. The constant error for observer Lk. is seen in this case to be slightly positive except for the largest movement, where it becomes negative. It never becomes greater than 22 mm. and the variable error, except in one case, is less than 25 mm. The lower half of the table gives the results for the blocked movements, in which the subject started to move along the track, knowing that at some point he would be blocked by the upright, but being in no case aware of the point at which the block was to occur. In these experiments the constant error is always positive, and becomes from two to eight times as large as in the case of the free movements. Indeed, in the case of the 100 mm. and 150 mm. movements, the positive constant error is larger than the original standard, meaning that the reproduced movement was more than twice as long as it ought to have been. As a consequence of this large constant error we come to deal with quantities of much greater magnitude than with the free standards, and the variable error becomes correspondingly larger, becoming now as large as 28 per cent., whereas before it never exceeded 15 per cent. It is obvious that under such conditions we are not studying the normal accuracy of movement, but are measuring the effect of impact on the perception of extent.

That this is true is shown conclusively in Table II. The purpose of these experiments, on another observer, was to discover in what degree the illusion is a function of the force of impact. At the bidding of the operator the observer started with the intention of moving one foot, two feet or three feet as the case might be. By this procedure the speed of the movement was varied quite uniformly, since

TABLE II

Influence of Force of Impact

Observer Hl.

Blocked at	10 cm.			20 cm.			30 cm.	
Intent to move.	1 ft.	2 ft.	3 ft.	1 ft.	2 ft.	3 ft.	2 ft.	3 ft.
Speed.	68	100	110	32	120	138	103	155
A.E. of speed.	3	2	6	9	7	5	9	8
C.E. per cent.	+138	+174	+171	+100	+158	+166	+90	+132
V.E. per cent.	*30*	*42*	*41*	*24*	*38*	*32*	*24*	*28*

large movements tend to be made more rapidly than smaller ones, and the variations of speed would of course make a corresponding variation of the force of impact against the upright. In some cases the movement was blocked at 10 cm., at 20 cm. or at 30 cm., at the option of the operator. As a matter of fact, a chance order was adopted throughout, care being taken that in the long run the same number of each kind was given. This number, as in the previous experiment, was 50. But the movement was not blocked in all cases. In 50 cases for each magnitude the subject was allowed to actually make the movement of 1 foot, 2 feet or 3 feet, which was his original intention. Then after the regular interval he went on to reproduce this movement. The records for these trials are given in Table III.

TABLE III

Showing Averages in mm. of Free Movements (1) Intended to Equal 1 ft., 2 ft. and 3 ft., and Averages of Reproductions (2) of these Free Standards, with Errors in Percentage

Observer Hl.

To move	1 ft.	2 ft.	3 ft.
Av. 1 (mm.).	217	371	485
Av. 2 (mm.).	259	385	454
A.E. per cent.	19	4	6
C.E. per cent.	+21	+6	—6
V.E per cent.	13	12	11

After having made these experiments, the actual speed at the various points of blocking, under the different conditions, was computed on the basis of ten movements of each of the standard magnitudes. The speed, in each case, is given in terms of mm. passed over during the twentieth of a second preceding and the twentieth of a second after the particular point of blocking in question.

Examination of the tables discloses several points of interest. Thus, in Table II., reading across on the level of any one block point, as at 20 cm. under 1 foot, 2 feet and 3 feet, the positive constant error is seen to increase directly with the force of impact as indicated

in terms of speed or velocity, + 100 at speed 32, + 158 at speed 120 and + 166 at speed 138. Whether this increase is proportional or not can not easily be made out, because, since the continuous method was used in the reproductions, the second movements were in each case subject to the negative error pointed out in the chapter on the influence of the degree of contraction (Chapter VII.). This of course means that the positive error is in all cases really greater than it appears from the record, since, in addition to producing a positive error, it has counteracted the normal negative error.

The speed curve of ordinary movements of a given extent has been found to be rather uniform and typical.[1] The movement begins gradually and increases in velocity until about the middle of the extent, slowing down again as it approaches the end. "In all cases the middle point of the extent coincides almost exactly with the mid-point of the duration." Moreover, the maximum speed attained in executing a normal long movement is higher than that of an equally normal movement, made under the same circumstances but of less extent. The average speeds of the movements in the present experiments were found to be for the 1-, 2- and 3-foot standards, 70, 103 and 113 mm. respectively, per tenth of a second. Thus, when the observer intended to make a movement of 2 feet, the speed at 10 cm. averaged 100 mm. per .1 sec., with a M.V. of only 2 mm. At 20 cm. the movement had attained a speed of 120 mm. with a M.V. of 7 mm., while at 30 cm. the speed was decreasing as the movement approached its goal, averaging, at this point 103 mm., with a M.V. of 9 mm. The 20-cm. point would thus seem to be approximately the mid-point of the movement, although the subject felt himself to be making a 2-foot (about 60 cm.) movement. If we refer to Table III. we find this to be really the case, since the average attempt to make a 2-foot movement averaged a little over 37 cm., and half of this extent does not take us far short of the 20-cm. point. Similarly the one-foot movement is slowing down at 20 cm., while the 3-foot movement is still increasing in speed at 30 cm.

Reading across under the corresponding columns of the three sections of Table II., the constant error seems to be rather independent of the speed at the different block points. Thus in the 2-foot column, when blocked at 10 cm., the C.E. is + 174; blocked at 20 cm., with higher speed (120), the C.E. is only + 158, while at the 30-cm. block, although the speed is still 103, the C.E. is but + 90. Similarly, in the 3-foot column the blocks at 10, 20 and 30 cm., with increasing speeds of 110, 138, 155, the C.E. decreases

[1] Binet and Courtier, *op. cit.*, 664.

through +171, +166, +132. Although the errors are always positive, and strikingly so, in these vertical columns the greater error may occur when the speed is least. This seems to indicate that the same or a greater impact may mean, nevertheless, a smaller illusion, according as it stands near to or far from the end of the movement, but that it always means an illusion. The greater the amount of the movement already accomplished, the smaller the illusion. The indication seems to be that a movement checked by the block method half way towards completion is not the same thing mentally as a movement half as large as the original one, but at the same time a unit, beginning and ending under control. The first movement contains a variety of elements of distraction, chief of which are the original intention and the effect of impact.

The magnitude of the illusion seems thus to bear no exact mathematical relation to the force of impact, but to be highly complicated by other factors when such are present. But, other things being equal, the dependence seems to be direct and proportional. It is at least sufficiently clear that some method other than that of the block should be used in the study of movements. Consequently, in the experiments to follow, the signal method, already described (Chapter I.), is to be used. This method eliminates the elements of distraction and illusion, while at the same time enabling easy variation and control of the magnitude of the standard extent. All movements studied are then unitary movements, and can be properly compared with any other free movement.

There seems to be no possibility of making a general statement that will express in quantitative terms the effect of practise on the magnitude of an illusion of perception. Thus in recent contributions we find these two statements: "Practise affects the variable error but not the constant error"; "Practise decreases the magnitude of an illusion." Now a constant error is an illusion. Illusion takes place when an experience is taken to be what it is not. In these constant errors of movement we have just such a situation. An extent is estimated to be what it is not, and this seems to signify that some internal event, process or effect is also misjudged. It may be of interest to observe the effect of practise on the illusion of impact. Table IV. shows the result of seven days' practise, by another observer, without knowledge of results, and of seven days' later practise with knowledge. The procedure here was the same as in the former experiments, except that after the seventh day the observer was told immediately after each reproduction whether his second movement was "too short," "right" or "too long." (This was only in the case of the blocked movements.) No

statement of the amount of the error was made. Each record in the table is the average of five trials for the particular magnitude on the day in question. Through the first week the constant error is seen to have increased quite uniformly from day to day, practise, in the sense of repetition, instead of decreasing the illusion having just the reverse effect. The V.E., however, remained, on the whole, rather constant.

TABLE IV

EFFECT OF PRACTISE ON THE IMPACT ILLUSION

Observer V.

Average of Five Trials for each Magnitude.		100 mm. Fr.	100 mm. Blk.	150 mm. Fr.	150 mm. Blk.	200 mm. Fr.	200 mm. Blk.	250 mm. Fr.	250 mm. Blk.	300 mm. Fr.	300 mm. Blk.	1907 Dec.
Without Knowledge.	C.E.	13	89	29	63	25	75	19	31	40	27	26
	V.E.	*11*	*20*	*10*	*14*	*8*	*23*	*9*	*10*	*7*	*7*	
	C.E.	24	171	41	111	15	51	10	35	7	22	27
	V.E.	*9*	*30*	*10*	*25*	*15*	*11*	*8*	*3*	*4*	*9*	
	C.E.	14	137	20	94	21	69	12	37	—13	33	28
	V.E.	*17*	*40*	*8*	*13*	*6*	*3*	*10*	*16*	*6*	*8*	
	C.E.	37	150	37	83	11	56	30	47	9	28	29
	V.E.	*14*	*16*	*7*	*24*	*10*	*6*	*7*	*11*	*7*	*6*	
	C.E.	30	154	28	92	27	48	43	43	26	41	30
	V.E.	*13*	*27*	*2*	*21*	*4*	*15*	*8*	*11*	*13*	*10*	
	C.E.	59	99	47	85	29	53	19	43	20	29	31
	V.E.	*19*	*25*	*7*	*15*	*8*	*5*	*11*	*12*	*6*	*10*	
	C.E.	43	123	50	97	43	63	28	47	15	34	Jan.
	V.E.	*18*	*44*	*5*	*26*	*6*	*19*	*5*	*5*	*7*	*10*	1
With Knowledge.	C.E.	37	63	44	60	32	31	30	2	13	7	2
	V.E.	*13*	*27*	*8*	*29*	*17*	*12*	*8*	*11*	*8*	*14*	
	C.E.	19	45	20	45	17	27	5	13	4	— 9	3
	V.E.	*6*	*32*	*13*	*10*	*11*	*10*	*5*	*10*	*16*	*8*	
	C.E.	10	74	34	43	26	7	15	5	5	— 5	4
	V.E.	*9*	*23*	*6*	*22*	*6*	*8*	*6*	*6*	*5*	*4*	
	C.E.	26	54	13	31	13	1	13	—11	16	— 6	5
	V.E.	*12*	*29*	*1*	*8*	*6*	*5*	*5*	*9*	*10*	*7*	
	C.E.	16	70	11	19	6	12	12	— 1	6	— 4	6
	V.E.	*8*	*26*	*6*	*12*	*5*	*4*	*4*	*16*	*3*	*12*	
	C.E.	16	63	24	21	19	18	23	8	6	— 5	7
	V.E.	*10*	*13*	*18*	*11*	*8*	*15*	*9*	*5*	*5*	*9*	
	C.E.	24	47	35	25	21	6	12	— 8	4	—10	8
	V.E.	*9*	*18*	*6*	*12*	*3*	*11*	*6*	*11*	*8*	*9*	

During the second week the effect of knowledge was simply to shorten all reproductions. For the shorter movements the C.E. thus became smaller but remained positive throughout, while the previous positive error for the long movements became decidedly negative.

Apparently the effect of practise, as well as of knowledge, is not to decrease the illusion, but to provoke a deliberate shortening of the reproductions against the observer's own judgment. The C.E. for

TABLE V

Effect of Practise on the Impact Illusion

Observer Chr.

	Average of Five Trials for Each Magnitude	100 mm.		150 mm.		200 mm.		250 mm.		300 mm.		1907.
		Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Dec.
With Knowledge.	C.E.	31	224	12	141	24	157	9	75	— 1	69	1
	V.E.	*6*	*24*	*4*	*44*	*13*	*14*	*11*	*9*	*7*	*17*	
	C.E.	13	214	25	90	28	113	— 5	52	2	43	2
	V.E.	*6*	*20*	*13*	*18*	*9*	*29*	*8*	*21*	*6*	*20*	
	C.E.	7	99	— 4	52	10	25	— 3	22	— 1	9	3
	V.E.	*14*	*18*	*10*	*19*	*13*	*4*	*6*	*12*	*7*	*5*	
	C.E.	— 3	39	0	54	0	40	0	17	10	2	4
	V.E.	*13*	*15*	*17*	*30*	*12*	*13*	*7*	*10*	*16*	*8*	
	C.E.	2	88	9	24	7	35	— 4	18	— 7	13	5
	V.E.	*10*	*42*	*8*	*17*	*18*	*34*	*9*	*19*	*7*	*8*	
	C.E.	1	52	1	37	— 1	3	—14	4	2	— 3	6
	V.E.	*13*	*40*	*14*	*8*	*10*	*5*	*4*	*4*	*6*	*11*	
	C.E.	7	64	2	46	—10	31	— 5	25	3	2	9
	V.E.	*13*	*40*	*11*	*16*	*5*	*7*	*12*	*11*	*7*	*14*	
	C.E.	—12	18	—12	33	— 2	10	— 5	9	— 1	1	10
	V.E.	*6*	*23*	*7*	*16*	*7*	*13*	*5*	*6*	*7*	*9*	
	C.E.	—11	45	— 9	31	— 1	10	— 1	5	— 9	0	11
	V.E.	*11*	*27*	*9*	*14*	*7*	*11*	*4*	*6*	*2*	*9*	
	C.E.	—25	27	1	29	2	26	— 2	2	—11	0	12
	V.E.	*10*	*12*	*15*	*11*	*6*	*11*	*8*	*7*	*3*	*7*	
	C.E.	—23	21	— 6	8	— 3	8	— 1	— 6	— 4	—14	13
	V.E.	*4*	*16*	*12*	*8*	*7*	*9*	*6*	*8*	*3*	*6*	
	C.E.	6	27	1	18	4	9	— 4	4	3	3	14
	V.E.	*11*	*21*	*6*	*19*	*4*	*8*	*8*	*8*	*5*	*3*	
	C.E.	— 1	16	20	44	10	19	0	7	—15	1	16
	V.E.	*4*	*19*	*9*	*13*	*12*	*13*	*4*	*5*	*3*	*3*	
	C.E.	— 4	26	— 2	28	2	19	2	— 4	5	— 5	17
	V.E.	*8*	*11*	*6*	*14*	*5*	*12*	*10*	*7*	*8*	*10*	
	C.E.	— 5	1	9	9	3	5	— 2	— 7	1	1	18
	V.E.	*5*	*5*	*12*	*7*	*8*	*10*	*17*	*4*	*4*	*5*	

the long movements, which was less proportionately than that for the shorter ones, was shortened sufficiently to be transformed into a negative error. Here again the V.E. remains little changed throughout. Table V., for still another observer, for two weeks, with corrective knowledge from the beginning, and five daily trials for each

record, for both free and blocked movements, shows much the same effect. The variable errors remain practically unchanged, the positive constant errors for the blocked movements become quickly reduced, while the constant errors for the free movements, beginning as positive, soon become almost entirely negative. The real illusion still persists, and the deliberate attempt to correct it miscarries in producing an opposite error for the free movements. Another experiment, on a fifth observer for fourteen days shows the same persistence of the illusion and the same disastrous effect of the deliberate corrective attempts.

The effect of these corrective attempts on the reproduction of free movements is not unlike the suggestive results of Solomons'[2] experiment on two-point discrimination, and seems to throw some light on the nature and basis of the judgment of extent. Solomons' experiment demonstrated the susceptibility to suggestion of the "judgment of twoness" and its lack of connection with judgments of area, position, etc. These facts seemed to indicate that the judgment is at bottom but a matter of simple association. "We learn that a certain kind of sensation means two points, just as we learn that certain marks mean the letter H, that another group of sensations means "book," etc. In the experiment referred to the two-point and one-point touches were purposely made to differ in two other features—mode of application and locality—the two-point touch being made by a sharp blow, in one area, the one-point being applied more by pressure and always in another area. After a period of practise the conditions were reversed—"the double points now pressed down and in the place where the single point was formerly applied, while the single touch is made with a blow and in the place where at the start the double touch was made." Under these circumstances the judgment was reversed—two is called one and one two. "The peculiarities of the sensation due to the method of application and the locality, have completely superseded those due to the number of points, as a basis for the judgment." His conclusion is that any cutaneous sensation may give rise to a perception of two contacts if the past experience of the individual has established the proper associations, and that there seem to be reasons for supposing that the same holds for other cutaneous judgments—position, area, etc.

Our present experiment affords indications of a similar associative and empirical basis for the judgment of extent of movement. In the beginning of the experiment the movements of the subject were made on some already present basis of comparison—a certain

[2] *Psychol. Rev.*, 4, 246, 1897.

movement in one region of the arm's total possible swing was felt as equal to a certain other movement. Introspectively the basis satisfied the demands of the experiment. But objectively the impact disturbance induced striking discrepancy in the judgments of equality. So long as this discrepancy entailed no serious consequence the old system of criteria persisted and the error went unperceived. But as soon as the subject became aware of the large constant error in his reproductions, the desire for objective equality led to a transformation of the basis of judgment. The old signs of magnitude could no longer be relied on. At the end of the second week this new basis had become fairly well established and the accuracy of reproduction of the impact movements approximates the original accuracy of the free movements. The effect of this newly established basis on the judgment of free movements is significant. The correction which is appropriate in the case of impact movements is not restricted to these only, but is carried over into the other situation. This is most clearly shown in Table V., but appears also in the case of the larger movements in Table IV. Movements in the first part of the arm's swing, the standard extents, are the same. But the scale of criteria of extent in the further portion of the arm's swing has been shifted downward, an objectively shorter movement having been learned to be the equivalent of the standard extent. When these standards become free movements the newly acquired scale continues to be utilized. Since no correction was made in the case of these free movements, we may suppose that this scale would in its turn persist until objective necessities, awareness of error, or, in case the impact movements were dropped out, the gradual reassertion of the older and more firmly established system, led to modification in one direction or another. Such results, along with those of Solomons, not only tend to lead to an empirical theory of space perception, but persuade one to go the empiricist one better. Judgments of extent of movement do not seem to be dependent on an anatomically conditioned topographical relation between points on sensitive membranes (joint linings) and points in external space, or on any fixed serial order of stimulations of skin, tendon or muscle. As was the case with the judgment of twoness—the judgment of equality of extent seems to be at bottom a matter of simple association—those movements are judged to be equal which have been *learned to be* equal—any sensation quality which adequately identifies or differentiates a given movement being sufficient to serve as basis for the judgment of the equality or difference of this movement and any other movement with a similarly adequate and equally well

learned sensation quality.[3] The significance of this associative basis of equivalence will be further discussed in Chapter VII.

The cause of the impact illusion is not very clear. Three contributory factors seem to be present: (1) the original intention, (2) the irradiation of the stimulus, (3) the shock of impact, as a sensation in its own right.

1. *The Original Intention.*—This first factor is probably an important one. The subject sets out to make a movement of, say, two feet, and is blocked at 10 cm. Now the process of preparing for, innervating and beginning a movement of two feet is not just like any other experience. It requires a particular attitude, a particular more or less widely spread adjustment, and a particular operation of visual and motor imagery. It seems quite likely, then, that in the reproduction, the observer tends not so much to move over the distance he was allowed to go before, but to repeat the original performance—to take the same general attitude, and make the same innervation. And, since there is no block in the way, the reproduction tends to approximate the original intention of the first movement, and the resulting error is always positive. This explanation might be sufficient if the illusion occurred only in such cases. But in the case of the four subjects in which there was no such explicit intention we find the same error manifested. In all four of these cases the observer was simply told to move his finger along the track, knowing that at some point his movement would be blocked. The shock of impact might be expected at any moment, and the only intention present was to keep on moving until the shock came. And this mild intention is certainly inadequate to account for a positive constant error of 155 per cent. of the standard.

2. *Irradiation.*—The irradiation of the stimulus of the shock of impact may have caused the articular surface to be stimulated farther on, at points where it would have been stimulated had the movement actually been of greater magnitude. Strict adherents of the joint sense as the basis of judgments of extent of movement might find here a possible explanation of the illusion. Or the irradiation need not be conceived as restricted to the articular surfaces. Tensions, strains, compressions and various local signs of a qualitative or intensive kind are doubtless provoked in adjacent and outlying regions of the muscles, tendons and skin as well, and these, being ordinarily associated with greater movements, may assist in producing the present illusion.

3. *The Sensation Itself.*—The illusion may come under the gen-

[3] Messenger (*Psych. Rev.*, Monograph 22, 1903) seems to find a similar basis for the perception or recognition of number.

eral head of the phenomena of fusion, the shock of impact fusing with the perception that is uppermost in consciousness, increasing its sensory elements and thus the apparent magnitude of its object. The influence of a secondary stimulus in producing an apparent increase in a primary stimulus is a common experience. In the case of vision this influence has been found to be proportional to the intensity of the secondary stimulus.[4] We found this to be in general the case in this illusion. The effect of such an influence is always "the tendency to fusion of two or more sensations which are simultaneously experienced." The extent of a movement and the force of a blow may seem at first thought to be not only incommensurable but incapable of summation, but both are equally perceptions of magnitude, and Woodworth has shown that "there is a certain amount of correlation between the extent of the preliminary movement and the force of the blow."[5]

Whatever explanation we prefer, the significance of the illusion in the study of the accuracy of movement is clear. Thus in Angier's recent study[6] he finds that passive movements are more accurately perceived than are active movements. Earlier investigators found the reverse to be true. Now from the description of the method used in his experiments, it appears that Angier's results, in the case of active movements, were subject to this error produced by impact. It is then quite conceivable that the error of the active movements should be unfairly increased until it exceeded that of the passive movements, although under similar or equally favorable circumstances just the reverse might have been obtained. Münsterberg[7] used the same method for controlling the standard in his experiments in sense memory, and all but one of his subjects showed extremely large positive constant errors. In the case of the smallest magnitudes, 5 cm., this positive error was nearly always 100 per cent. or slightly less, decreasing rather uniformly with increase in the standard magnitude. The statement of the amount of error under such circumstances can not be said to express the fidelity of the memory for sensations of movement. Even if the C.E. is eliminated, the V.E. will be too great by virtue of the greater magnitudes involved. Besides, one is, in such an experiment, measuring not only the memory for extent as such, but at the same time the rate of decrease in vividness of the illusion.

[4] H. J. Pearce, "Law of Attraction in Illusion," *Psychol. Rev.*, **11**, 43, 1904.

[5] "Vol. Control of Force of Movement," *Psychol. Rev.*, **8**, 350–9, 1901.

[6] *Zeit. f. Psychol.*, **39**, 430, 1905.

[7] *Beiträge*, **4**, 69–88.

CHAPTER III

The Indifference Point

By the "indifference point" is meant the point in a scale of magnitudes at which there is no constant error of estimation. When estimates or reproductions of such magnitudes are attempted the general rule is that the smaller are judged or reproduced too large while the greater are underestimated. At some mean magnitude only the variable error is found, and judgments at this point are consequently more precise. The region about this mean magnitude has been called the "indifference" point, more properly, the region of indifference. The phenomenon of the "indifference point" seems to have been first observed in experiments on the time-sense. Vierordt,[1] writing on the basis of Camerer's experiments, found that "there is an unexceptionable law that small intervals are overestimated and reproduced so on the kymograph, whereas longer times are inevitably shortened." Vierordt also states that the "indifference point" is not absolutely fixed, but varies with different individuals and at different times in the same individual. "It depends especially on the conditions of the experiment, as well as on the sense investigated." But the "conditions of the experiment" were not specified, and it will be seen later that Vierordt himself was misled by disregarding them.

From the time of Vierordt the long array of investigators of the time-sense set themselves the problem of the constant error, and sought to find an indifference point which would be the true one. Höring,[2] Kollert,[3] Estel,[4] Glass,[5] Nichols,[6] Schumann[7] and Stevens,[8] in turn, found regions of indifference, but at varying points in the scale, *e. g.*, Höring at about .5 sec., Kollert at about .8 sec., Glass at 2 to 5 sec., Nichols at about 1 sec. and Stevens .7 sec. on one occasion and 3 sec. on another. Periodically recurring "indifferent points" were asserted and denied, and numerous attempts made to relate the unit of periodicity to various bodily processes, such as

[1] "Zeitsinn," 1868, p. 17.
[2] Dissertation, Tübingen, 1864.
[3] *Phil. Stud.*, **1**, 78, 1882.
[4] *Ibid.*, **2**, 37, 1884.
[5] *Ibid.*, **4**, 423, 1887.
[6] *Amer. Jour. of Psychol.*, **3**, 453, 1890.
[7] *Zeit. f. Psychol. u. Sinn.*, **2**, 294, 1891.
[8] *Am. Jour. of Psychol.*, **13**, 1, 1902.

breathing, pulse, swing of leg, etc. These points will be more fully referred to after the present experiment is described. But the futility of attempting to relate the I.P. (as we shall hereafter designate the "indifference point") to the temporal periods of organic processes should have become apparent as soon as it was found to be a characteristic of all our judgments of serial magnitudes, both temporal and non-temporal. Vierordt had suggested that "a similar relation is to be found in our spatial judgments," but since the temporal relations of motor processes play so large a part in our spatial judgments, it may well have been supposed that the constant error in the case of space magnitudes is "simply the consequence of the rapidity of movement—hence a phenomenon of temporal estimation as well."[9]

But the I.P. is also found in judgments of weight, force and brightness, as well as in those of time and extent. In all these fields, again, there is little agreement among investigators, though there is usually a tendency to speak of the I.P. as though it were in each case some absolute and fixed region. In the estimation of force the I.P. is variously placed at from 200 to 1,600 grams. Cattell and Fullerton, using seven observers, with a series ranging from 200 to 1,600 grams, found the I.P. to be in all cases between 400 and 800 grams. Wreschner, studying the perception of lifted weights, found an I.P. at 1,200 grams and generalizes by saying that high intensities weaken in the memory while low ones are strengthened, a certain moderate intensity remaining unchanged, in both one-hand and two-hand experiments. Leuba,[10] experimenting with memory for brightness intensities, finds a striking difference between the ratios at the lower and upper ends of the scale. "There seems to be a natural tendency to shift the sensation held in memory towards the middle of the scale of intensities." In other words, low lights are overestimated while high ones are under-rated. The recent work of Lewis[11] gives some evidence in confirmation of Leuba's results.

All experimenters on the extent of movement seem to have found regions of indifference, flanked above and below by negative and positive constant errors. And although these I.P.'s all differ among themselves, it has still been the custom to refer to the indifference point as thought it were an absolute something. Thus Kramer and Moskiewicz and Jaensch surmise that there is such a thing as a "most favorable" extent, as well as a "most favorable"

[9] Külpe, "Outlines," 343.

[10] *Amer. Jour. of Psychol.*, 5, 370, 1892.

[11] Johns Hopkins Studies, No. 2. *Psych. Rev.*, Mon. Supp., No. 40, 55, 1909.

time. Schneider,[12] working with distances ranging from 70 to 100 mm. finds an I.P. at 90 mm., Delabarre[13] locates it at about 300–400 mm., Falk[14] at 70–80 mm., and Münsterberg at 100–200 mm.,[15] while Cattell and Fullerton[16] find it to be 100 for one observer, 300 for another and 600 for a third. It was the disparity of these results in the extent of movement which suggested the present experiment, for while differences of a fraction of a second in the case of experiments on time may be considered quite possibly due to individual, mechanical and experimental conditions, the difference between 70 mm. and 600 mm. within the possible range of horizontal arm movements calls for some other explanation.

That no such explanation has been suggested is shown by the fact that in the most recent and thorough review of the subject of movement these disagreements are merely stated,[17] without being brought under any general law. There is still the tendency to treat the I.P. as a fixed magnitude of yet undetermined location, without specific regard to the series in which it occurs, although Vierordt long ago remarked that the actual magnitude of the I.P. depended on the "category" in which it was placed.

The present attempt to demonstrate the general law for the appearance of the I.P. phenomenon grew out of experiments on the accuracy of reproduction of active and passive movements. In this experiment small magnitudes were employed, the particular lengths ranging from 43 to 100 mm. In nearly every case, both for the active and the passive movements, a constant error was found, which was positive for the small magnitudes and negative for the relatively large. The region of indifference was found between 60 and 75 mm., falling at about the middle of the series. These figures, taken alone, tend to confirm Falk's statement. But another series of experiments with the Cattell-Fullerton apparatus, using a range of 100–300 mm., resulted, in several subjects, in a quite uniform I.P. at about 200 mm., thus agreeing with Münsterberg, who used a similar apparatus with about the same magnitudes. Nevertheless, the subjects of Cattell and Fullerton, attempting to reproduce from memory the extents used in their experiments, 100, 300, 500 and 700 mm., showed an I.P. between 300 and 500. The inference that the difference here found was purely a function of the series seems

[12] "La Memoire des Mouvements Actifs," Diss. Juriew, 1894.

[13] "Bewegunsemfindungen," 89.

[14] "Räumschätzung mit Hülfe von Armbewegungen," Diss. Dorpat, 1890.

[15] *Beitrage*, 2, 159.

[16] *Op. cit.*, 51.

[17] Woodworth, "Le Mouvement," Chapt. VI.

obvious. Yet in the field of the time sense as well as in the field of movement, there had been, since the days of Vierordt and Camerer, attempts to find the real "indifference point," each investigator using such series limits as seemed to suit his convenience, apparently with no suspicion that the I.P. might be purely a function of the upper and lower limits of the scale of magnitudes in the particular experiment.

In order to test this suspicion in the field of movement, the following experiment was made. A standard series of magnitudes was chosen, ranging from 10 mm. to 250 mm., and this series was divided into three sections, A, B and C. The magnitudes of section A ranged from 10 mm. (increasing by increments of 10 mm.) to 70 mm. Those of section C, from 70 to 250 mm., with increments of 30 mm., and those of section B, from 30 to 150 mm., with increments of 20 mm., thus overlapping the inner ends of sections A and C. There were thus seven standard magnitudes in each section, and the three sections, A, B and C represented respectively the lower, middle and upper regions of the total scale (S) originally selected. The standards were made by cutting a narrow slit of the desired length in one strip of cardboard and pasting this strip upon another such strip as foundation. This formed a furrow which served as adequate guide for the stylus of a Delabarre pendulum planchette, or for the blunt point of a heavy carbon pencil which might be held between the thumb and fore-finger while the forearm rested on the planchette board. The furrow was carefully rubbed with graphite, thus affording a smooth and noiseless track.

Experiments were now designed with the following purposes in mind. (1) To see whether a periodic I.P. could be found within the total series (S), by working with its special sections, finding an I.P. in A, one in B and another in C. (2) To see, for instance, whether the same absolute magnitude might be, under one circumstance an I.P., under another circumstance affected with a positive constant error, or again, with a negative constant error. (3) To ascertain whether the gradual extension of the series limits would be accompanied by a corresponding change in the position of the I.P. (4) To find whether any magnitude in the series evinced any constant error when estimated out of relation to a series or section. (5) To learn whether or not the C.E.'s occur, even in serial experiments, when the separate trials are distributed over a considerable period of time.

Procedure.—A Delabarre pendulum planchette, with a 14-foot radius, was suspended over the outer edge of a writing table of

comfortable height. The observer sat with his left forearm on the board, with the carbon pencil held between thumb and forefinger. The position of the hand was made constant by inserting the end of the little finger in the pen shaft of the board. The observer wore the blind mask throughout or worked with closed eyes. The board swung an inch and a half clear of the table, on which lay several large sheets of smooth white paper. One of the standard strips was now placed underneath the board and the point of the carbon pencil inserted at the beginning of the guide slit. The cards of varying magnitudes were always placed so that the center of the board, when the pendulum was at rest, was directly over the halfway mark of the slit. The observer could now trace the path with a minimum of interference and adjustment, and with practically a horizontal swing of the forearm, the actual movement being thus rectilinear, a compound of elbow and shoulder joint flexion. After tracing the slit, the carbon point was brought back to the initial position for the respective magnitude and after an interval of two seconds an effort was made to reproduce the magnitude, the cardboard strip having been removed by the operator and the carbon point writing on the smooth white paper. After another interval of about two seconds, a second attempt was made, in which the observer estimated the probable or apparent error of his first trial and tried to reproduce the original magnitude more exactly.

TABLE VI

Error of Reproduction, First and Second Trials and their Average

Obs.	Trial.	10		20		30		40		50		60		70	
		C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
R.	1	+2	*2*	+ 2	*5*	0	*6*	—4	*6*	—6	*6*	—13	*8*	—16	*7*
	2	+4	*3*	+ 6	*5*	+5	*6*	+1	*8*	—3	*8*	— 6	*8*	—12	*8*
	Av.	+3	*2.5*	+ 4	*5*	+2.5	*6*	—1.5	*7*	—4.5	*7*	— 9.5	*8*	—14	*7.5*
B.	1	+5	*2*	+ 7	*5*	+3	*6*	+2	*5*	+1	*5*	— 3	*8*	— 8	*8*
	2	+6	*3*	+10	*5*	+5	*5*	+5	*5*	+4	*6*	— 1	*8*	— 3	*7*
	Av.	+5.5	*2.5*	+ 8.5	*5*	+4	*5.5*	+3.5	*5*	+2.5	*5.5*	— 2	*8*	— 5.5	*7.5*
C.	1	+3	*4*	+ 2	*4*	+2	*6*	—5	*6*	—2	*6*	— 6	*8*	—12	*9*
	2	+1	*3*	+ 2	*4*	+4	*6*	+1	*7*	+1	*9*	— 6	*7*	—10	*8*
	Av.	+2	*3.5*	+ 2	*4*	+3	*6*	—2	*6.5*	— .5	*7.5*	— 6	*7.5*	—11	*8.5*
Av.		+3.5	*2.8*	+ 4.8	*4.6*	+3.2	*5.8*	0	*6.3*	— .8	*6.6*	— 5.8	*7.8*	—10.2	*7.8*

At a given sitting but one section, A, B or C, was used, each of the seven magnitudes being presented in chance order, but no magnitude being repeated until all of the others of the series had been presented. Twenty-five trials for each magnitude were taken, making, with the corrective attempts, fifty reproductions for each of

the twenty-one standards, a total of 1,050 movements for each subject. Three subjects were used, all being graduate students in psychology, two of them ignorant of the purpose of the experiment, and all three ignorant of the results from day to day.

TABLE VII

ERROR OF REPRODUCTION

Obs.	Trial.	30		50		70		90		110		130		150	
		C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
R.	1	+ 1	4	+ 1	8	—8	10	—10	8	—20	11	—29	7	—30	11
	2	+ 5	5	+ 5	9	+2	9	— 7	10	—15	9	—27	8	—30	10
	Av.	+ 3	4.5	+ 3	8.5	—3	9.5	— 8.5	9	—17.5	10	—28	7.5	—30	10.5
B.	1	+ 8	6	+ 9	11	+3	13	— 3	9	—10	12	—14	12	—15	12
	2	+12	6	+16	10	+5	13	— 1	14	— 9	9	—11	12	—14	14
	Av.	+10	6	+12.5	10.5	+4	13	— 2	11.5	— 9.5	10.5	—12.5	12	—14.5	13
C.	1	+ 6	7	0	7	+3	8	— 1	8	— 6	10	—14	14	—14	14
	2	+ 7	9	+ 3	8	+5	9	— 2	8	— 5	14	—16	11	—17	14
	Av.	+ 6.5	8	+ 1.5	7.5	+4	8.5	— 1.5	8	— 5.5	12	—15	12.5	—15.5	14
Av.		+ 6.5	6.2	+ 5	8.8	+1.7	10.3	— 4	9.5	—10.8	10.8	—18.5	10.7	—20	12.5

TABLE VIII

ERROR OF REPRODUCTION

Obs.	Trial	70 mm.		100 mm.		130 mm.		160 mm.		190 mm.		220 mm.		250 mm.	
		C.E.	V.E.	C.E.	V.E	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
R.	1	+14	9	0	10	—12	10	—23	13	—37	11	—53	15	—68	19
	2	+29	12	+11	12	0	10	—17	12	—30	16	—55	15	—73	15
	Av.	+22	11	+ 5.5	11	— 6	10	—20	12.5	—33.5	13.5	—54	15	—70.5	17
B.	1	+10	11	+10	9	— 1	14	—16	15	—16	14	—25	14	—38	18
	2	+17	12	+14	8	— 1	12	—19	14	—17	12	—32	19	—42	18
	Av.	+13.5	11.5	+12	8.5	— 1	13	—17.5	14.5	—16.5	13	—28.5	16.5	—40	18
C.	1	+12	8	+ 8	11	+ 3	14	— 8	14	—15	11	—33	18	—45	15
	2	+17	11	+14	15	+ 7	11	— 2	14	—12	14	—32	18	—47	16
	Av.	+14.5	9.5	+11	13	+ 5	12.5	— 5	14	—13.5	12.5	—32.5	18	—46	15.5
Av.		+16.5	10.5	+ 9.5	10.8	— .6	11.8	—14.2	13.7	—21.2	13	—38.3	16.5	—52.2	17

Tables VI., VII. and VIII show the results for sections A, B and C for the three subjects, including the constant error and variable error of both the first and the corrective trials, and the average constant error and variable error of the two trials, for each subject, along with the grand averages for all three subjects. The unit throughout is the millimeter.

The results are thoroughly clear and uniform. In every section employed we find the relatively small magnitudes reproduced too great, and the greater magnitudes reproduced too small, both in the first and in the corrective attempts, while there is a region of in-

difference at about the middle of each section. Moreover, in the case of the small magnitudes the positive error in the corrective attempts is seen to be greater, in 24 averages out of the 26 in which the first error was positive, than the first error. The difference ranges in section A from 1 to 3 mm. (with average of $2\frac{1}{4}$, in B from 1 to 7 mm. (with average $3\frac{1}{2}$), and in C from 3 to 15 mm. (with average $7\frac{1}{2}$).

But in the case of the larger magnitudes we find that only in 10 cases out of the 37 averages in which the constant error is negative is the corrective error still more negative than the first. Of these cases, 7 come in section C, which contained the greater magnitudes. Thus, in 27 averages out of 37, although the constant error was always negative, the correction was positive, just as in the cases of the positive constant errors. These corrective attempts were introduced in order to see if there might not be an attenuation of accuracy, by virtue of the mere repetition of the process of judgment, the positive errors being thus made more positive and the negative more negative. But this is not the case. Instead all errors tend, on the whole, to become more positive, the tendency being more pronounced, however, in the case of errors already positive. It is probable, consequently, that this effect has nothing to do with the particular illusion which we are studying. The phenomenon is probably simply a "warming up" effect, the second movement being easier than the first by virtue of the motor inertia having already been overcome. This would tend to make the corrective movement really greater than it appeared. In the case of the first attempts, then, this fact would also have some bearing. It would mean that the negative errors, so far as actual judgment is concerned, are really greater, that is, more negative than they seem to be in measurement by an objective scale, and that the positive errors, similarly, are not really quite so great as measurement shows them to be.

Turning now to the real point of the experiment, we find in section A (10–70 mm.), for the three subjects, an average I.P. at about 40 mm.; in section B (30–150 mm.), an I.P. at about 75 mm., and in section C (70–250 mm.), an I.P. at about 125 mm. By some singular coincidence the ratio of the approximate I.P. to the upper magnitude of each section is in every case almost exactly one half. It is apparent that in extent of movement and in time of movement, so far as time is a function of extent, we can find an I.P. at whatever point we choose. Given the series of magnitudes with which we are to work, we may be quite certain that our region of indif-

ference will fall at about the mid-point or region of the particular scale. Thus in the present experiment we find that 70 mm., which is always underestimated in section A, falls within the region of indifference in section B, and is always overestimated in section C, etc.

TABLE IX

ERROR OF REPRODUCTION FOR ISOLATED MAGNITUDES

		10 mm.			70 mm.			250 mm.		
		1st.	2d.	Av.	1st.	2d.	Av.	1st.	2d.	Av.
R.	C.E.	— .6	— .3	— .5	+1.5	+4.6	+3	— .7	—2.4	—1.5
	V.E.	*1.6*	*1.5*	*1.5*	*6.6*	*6*	*6.3*	*12.3*	*15.3*	*13.8*
	+	11	13	24	11	13	24	11	13	24
	—	14	12	26	14	12	26	14	12	26
B.	C.E.	+ .3	+3	+3	+2	+2	+2	—3	—5	—4
	V.E.	*1.8*	*1.5*	*1.7*	*4.2*	*5*	*4.6*	*12*	*11*	*12*
	+	24	23	47	14	14	28	10	8	18
	—	1	2	3	11	11	22	15	17	32
C.	C.E.	— .2	— .3	— .3	—2	+2	0	—2	—4	—3
	V.E.	*1.5*	*1.3*	*1.4*	*6.7*	*6.3*	*6.5*	*19*	*23*	*21*
	+	14	13	27	12	13	25	12	14	26
	—	11	12	23	13	12	25	13	11	24

A second experiment, performed as a check to the preceding, shows the facts still more clearly. About four months after the sectional records had been taken, the three magnitudes, 10 mm., which had always been affected with a positive constant error, 70 mm., which fell now into a positive error, now into the I.P. and now into a negative error, and 250 mm., which was always underestimated, were used singly and on occasions several days apart, after the same method. Table IX. shows the results for the three subjects, giving the constant and variable errors for the first and the corrective trials, and their average, together with the number of positive and negative errors in each case. Comparing these constant errors with those of the same magnitudes in the previous experiment, the effect of inclusion in a series is evident. The magnitude 10 mm., always overestimated from 2 to 6 mm. in section A, is here slightly underestimated (from .2 to .6 mm. in the case of the two subjects who knew nothing of the purpose and results of the experiment), and it will be seen that this constant error is a purely chance one, since the number of positive and negative errors is almost equal—51 positive and 49 negative. The magnitude 70 mm., with average constant error of — 10 mm. in section A, + 1.7 mm. in section B, and + 16.5 mm. in section C, when taken by itself has a constant error that is purely accidental, the + and — cases

being here also almost equally divided, 25 + and 25 — in the case of C., 24 + and 26 — in the case of R., and 28 + and 22 — in the case of B., with average constant errors of 0, + 3 and + 2. The magnitude 250 mm. shows the same results, although the calculated constant error is slightly negative in all cases (grand average, — 2.8 mm.). The + and — cases show the same equal and chance distribution, in the cases of C. and R. 50 + and 50 —. Only in the case of B. is there the slightest deviation from this rule. But even here the constant error for 10 mm. is only + 3 mm. as against + 5.5 mm. in section A, that for 70-mm. only + 2 mm. as against + 13.5 mm. in section B, while that for 250 mm. is — 4 mm. as against — 40 mm. in section C.

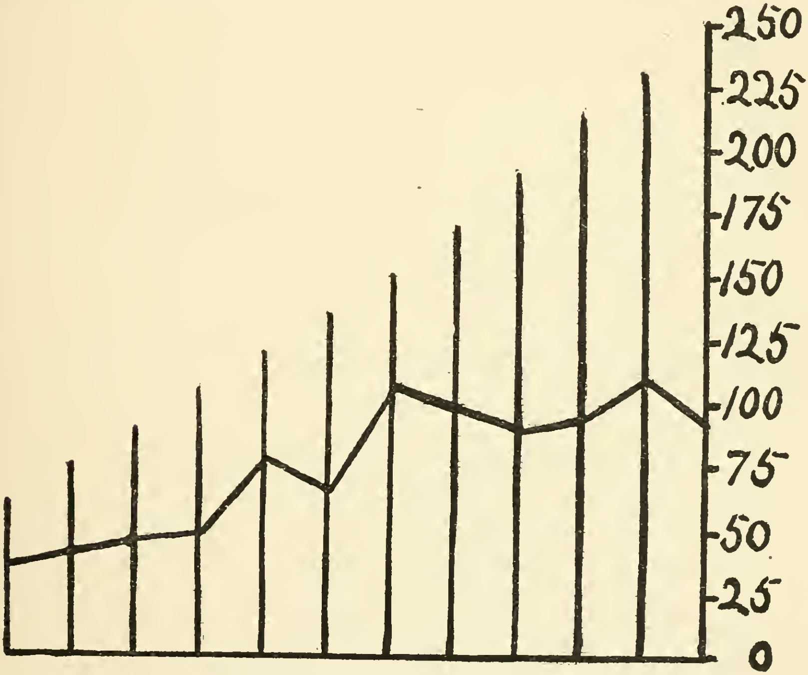

FIG. 1. Showing the Rise of the Indifference Point with the Extension of the Series Limits.

This conclusion is further justified by an interesting variation of the experiment, performed with another subject (Hp.) who was ignorant of the purpose and previous conduct of the experiment as well as of the general subject of the indifference point. A set of standard magnitudes was prepared ranging from 10 mm. to 60 mm. by increments of 10 mm., from 60 mm. to 150 mm. by increments of 15 mm., and on to 250 mm. by increments of 20 mm. The

standards of series 10–60 were now given and reproduced in chance order, five trials being made of each magnitude, a total of 30 trials. At this point, without the knowledge of the subject, the next magnitude (75) of the series was introduced, and again five trials made of each standard. Then the next magnitude was introduced and similar tests made for series 10–90. This process was continued until all the higher magnitudes of the series had been introduced, the last set thus consisting of five trials for each magnitude of the total series 10–250. This made a total of 690 movements. All these trials were made at a single sitting, so that the subject worked with a gradually expanding series, the lower limit of which remained constant while the upper limit increased by regular and approximately equal steps.

The results are shown in Fig. 1. In set 1 the series breaks, as usual, approximately half way between the two extremes, giving an I.P. at 35 mm., with positive constant errors below and negative above. In set 2 the I.P. rises to about 40 mm., in set 3 to 45 mm., in set 4 to 50 mm., and similarly throughout the whole experiment (disregarding the exceptional height of the I.P. for range 0–150), each new standard, as it is introduced, being found to influence the apparent magnitude of every other. The general effect of this influence is to increase all positive errors. This increase shows itself in three ways: (1) When the C.E. was positive from the beginning, this error is seen, on the whole, to increase with the introduction of each new standard. Thus the positive C.E. for 10 mm., at first 8.4, becomes as great as 13.6, that for 20 mm. increases from 3.8 to 14.8, and that for 30 mm., from 1 mm. to 10.8. (2) Constant errors which were in the beginning negative undergo a transformation in the course of the experiment. Each decreases in magnitude through an indifference point of its own, below which it emerges as a positive error. This is shown in the case of all standards from 40 mm. through 120 mm. (3) As a result of these transformations the region of indifference varies about a constantly augmenting magnitude which ranges from 35 mm. to 100 mm., but goes in two cases as high as 110 mm. and 120 mm. In general, the indifference point rises in response to the extension of the series at its upper limit, and lies, in all cases, at a point which represents roughly the median of the total group of magnitudes.

The results of all three experiments are consistent, and afford the following answers to four of our introductory questions. (1) A periodic I.P. can be found within the total series (S) by working with its special sections (A, B and C). (2) The same absolute magnitude may be under one circumstance an I.P., under another,

effected with a positive C.E. or again with a negative C.E. (3) The gradual extension of the series limits is accompanied by a corresponding shift in the region of indifference. (4) No magnitude evinces any C.E. when estimated out of relation to a series or group of which it is a member. (5) The reply to the fifth question is to be found in the tables of Chapter V., in which are shown the effect, on the direction and magnitude of errors of reproduction, of lengthening the interval between standard and reproduction. This procedure has the effect of extending the period of time within which the individual members of the series occur. We have found that in the case of 2-sec. intervals, with such series as were used in the present experiment, the influence of the judgments of one magnitude persists throughout the experiment, affecting in a very definite manner the judgments of all other magnitudes. Our present query is concerning the persistence time of such an influence. In the experiments of Chapter V. intervals of 2, 5, 10, 15 and 30 seconds were introduced between the standard and the reproduction. These experiments afford somewhat definite answer to our question. In Table XII., recording the errors in reproduction of extent for observer W., the I.P. is found in all series up to the 15-sec. interval. When intervals of 30 sec. are employed the C.E.'s are all considerably negative and the group effect is not found. In Table XV., recording the error in the reproduction of duration for the same observer, the I.P. appears after 2-sec. and 5-sec. intervals, but not beyond, while in similar experiments on observer H. (Table XVI.) the I.P. is found only after intervals of 2 sec. While these are but incidental observations and can not be said to serve as basis for any adequate quantitative statement, there is evident indication that the constant errors do not so much represent transformations in a memory image of the stimulus in question as they do the effect, on a present judgment, of the persistence of the mental set involved in a previous judgment. If the interval between the two judgments is sufficient, the first disposition is soon dissipated and is no longer adequate to affect the second performance. That the C.E. is not due to the transformation of a memory image is apparent from the fact that in this case no C.E. appears.

I conclude then that the phenomenon of the indifference point, so far as it occurs in our spatial judgments, and in our temporal judgments so far as they are a function of the extent of movement, is of purely central origin, and that its position depends entirely upon the range or limits of the magnitudes in question.

The suspicion is a natural one that the varying and contradictory I.P.'s found by the investigators of the time-sense were due to dif-

ferences in the limits of the series of magnitudes employed. And if the actual range of magnitudes used be but tabulated alongside the I.P.'s obtained, the suspicion is strikingly sustained. In many cases the range of magnitudes actually used is not clearly stated, nor is it always clear how much of the total range was used at a given sitting. Such data as could be found from those who particularly studied the constant error have been tabulated in the following.

TABLE X

RELATION OF I.P. TO RANGE OF INTERVALS

Investigator	*I. P., in Seconds*	*Range, in Seconds*
Vierordt.		
Subject N.	1.5	.5 to 5.8
" H.	1.4	1 to 3.5
" V.	3	.6 to 3.5
Touch.	2.5	.25 to 5
Hearing.	3.5	.5 to 8
Spontaneous movement.	5	.2 to 65
Höring.	.5	.3 to 1.4
Kollert.	.8	.4 to 1.8
Estel.	Multiples of .75	Used different sections of range 1.5 to 8 at different parts of day.
Glass.	2 to 5	.7 to 15
	3	.7 to 9
Stevens.	.7	.2 to 3

One would scarcely pretend to submit these figures to a thoroughgoing comparison, because the methods and apparatus were extremely varied, and the subjects as well as the operators were all different. Moreover, the influence on the I.P. need not be supposed to be directly proportional to the quantitative shift in the series limits. Nevetheless the table is suggestive. Vierordt, for instance, states, on the basis of the averages quoted in the above table, that the I.P. "depends especially on the sense under investigation," and finds that for touch the I.P. falls at 2.5 sec., for hearing at 3.5, and for the time of spontaneous movements at 5 sec. But another fact should be observed which Vierordt failed to point out, viz., that for touch the range of intervals employed was .25 to 5.0 sec., for hearing .5 to 8 sec. and for movement .2 to 65 sec. In the light of the results we have just reported it is extremely probable that the variation of the I.P. is much more a function of the range of intervals employed than of the "sense under investigation." For in every case the region of indifference approximates the middle of the range.[18]

[18] In the case of the spontaneous movements only 216 out of 1,708 movements were over 20 seconds in duration, and only 444 of the upper third of his scale contrast with 757 movements in the lower third. The I.P. would probably have been still higher here if both ends of the scale had been equally employed.

Similarly, Höring, with a range of .3 to 1.4 sec., finds his I.P. at .5 sec., Kollert, with a range of .4 to 1.8 sec., finds a higher I.P., and Stevens, with range .2 to 3 sec., finds it to be at .7 sec., while Glass, using range .7 to 9 sec., finds it at 3 sec., although for range .7 to 15 sec. it rises as high as 5 sec. With these facts in mind little stock can be taken in the reports of rhythmic, oscillating, periodic indifference points, unless it is explicitly stated just what range of intervals was employed, not merely in the experiment as a whole, but at each sitting from which results are used. Estel, who found periodic I.P. in his total scale, employed different sections of this scale at different sittings. Shaw and Wrinch,[19] who used the method of successive reproduction, thus partially eliminating the effect of range, do not state the order in which the different intervals were used. Negative errors were found for .5, .75 and 1.5 seconds. Records on four subjects, using intervals ranging from .9 to 10 seconds, did not agree.

Theoretical.—The cause of this tendency to positive and negative constant errors at the two extremes of a scale of magnitudes has always remained obscure. At first thought it seems to be but an instance of Fechner's time-error, with an unexplained difference in direction at the two ends of the scale. But that the illusion is not based on the temporal positions of the two stimuli is clear from the fact that it does not occur when the magnitudes are taken singly, although the temporal positions of standard and reproduction remain unchanged. Schumann attributed the phenomenon, in the case of time, to mechanical sources of error in the apparatus. But this could hardly explain the same tendency in judging intensity of light (Leuba, Lewis), or the size of squares (Kennedy) or visual lines (Münsterberg). Vierordt's suggestion was that it might be due either to the peculiarity of the sense organ or to the accompanying psychical processes. Delabarre,[20] working with extent of movement, offers various explanations, none of which seem adequate. The first suggestion is the lack of proper and sufficient current control in performing the short movements. The reproduction of the short movements is thus rougher, and the moving member can not be stopped at will. This is probably the case with the very small movements, but possibility of current control would seem to be a rather fixed physiological factor and not to vary up and down the scale of extension. This it must do if it is to account for the fact that a relatively large movement, underestimated in one series, is overestimated when placed in another series. The same criticism

[19] Univ. of Toronto Studies, 1, 105, 1900.

[20] *Op. cit.*, 89.

also applies to his statement that these small movements are unusual and hence overestimated. Actually, movements so small as writing movements are not at all unusual, and are made, moreover, with considerable precision, while we found movements of a foot or more being overestimated. Slower speed for the long movements is also suggested. While this might apply to the greater extents, it has no value for the cases of the other senses, in which the time element is not involved. In the same way objection must be made to the theory that the effect of the constant change and renewal of the motor impulse is to increase the apparent magnitude of the large movements. Besides, all these points apply to the standard movement as well as to the reproduction, and do not make clear why one should be affected in judgment, the other not.

Wundt considers the tendency to overestimate small articular movements and to underestimate large ones as a tactual analogue of the similar illusion present in the estimation of visual angles. "This comes under the general principle that a relatively greater expenditure of energy is required for a short movement than for a more extensive one, because it is relatively more difficult to begin a movement than to continue it after it is started."[21] If it were merely a case of estimation in terms of an objective unit, or even a case of the comparison of two arbitrarily given standards, Wundt's suggestion might suffice. But in all these experiments the method of reproduction has been used. The observer's task was not the comparison of one standard with another, but the reproduction of a given normal stimulus, and it was this reproduction that showed the constant error, and there is no reason for supposing the "relative difficulty" of a short movement to interfere with the attempt to reproduce the same short movement. The same "relative difficulty" is present in the reproduction as in the standard.

By far the most complete discussion of the positive and negative errors and the resulting indifference point in the field of movement is to be found in Woodworth's chapter[22] on the subject. The possible efficient causes are here analyzed into motor factors, sensory factors, emotional factors and factors of attention and association. Under motor factors are included the facts of dynamogenic stimulation and fatigue. If we suppose that the first small movement acts as a stimulant to the motor system, the reproduction would be expected to show the effect in being somewhat more easily made, hence probably correspondingly larger than necessary. And in the case

[21] Wundt, "Outlines," 3d Eng. ed., p. 139, 1907.

[22] "Le Mouvement," Chapt. VI.

of the negative errors it might be supposed that the longer movements, instead of acting as stimulants, actually have a fatiguing effect, so that their reproductions are somewhat more difficult and fall short. But this last supposition is shown to be "undoubtedly false," from the fact that a single contraction, of even maximal force, does not fatigue, but acts as a stimulant to the motor apparatus. Besides, the illusion does not become more pronounced in the course of the experiment, as should be expected on the basis of fatigue. Woodworth admits that the motor factor probably helps to produce the positive error, but insists that this factor alone is insufficient to explain any illusion of perception, since it still requires to be shown why the error, once made, is not perceived and allowed for.

Moreover, in the light of the present experiment, and the quoted results of many others, neither the stimulating effect of the small movements nor the fatiguing effect of large ones, nor both together, would suffice to account for the fluctuation of the indifference point with the change of series limits. It is hardly possible that a 70-mm. movement, taken in one series, should be physiologically stimulating, in another of slightly different range, fatiguing, while in still another it should be physiologically indifferent. For the same reason the exciting or depressing emotional effect of the preceding movement, while it might be conceived as contributing to the total effect if the indifference point were found to be fixed, can hardly be supposed to possess the same variability as is found in the indifference point.

To Woodworth, "the sensory factor in the genesis of the constant errors and illusions may seem the only one worth recognizing." Under sensory factors he includes the lack of adequate sensory evidence for finer discrimination of movements, and an inequality of peripheral sensation, shown in the fact that "the longer and stronger the habitual movements of a member, the less felt are its movements." While these factors seem to be of value in accounting for asymmetrical errors and in comparison of movements in different directions or by different parts of the body, they do not seem to have any direct bearing on the question of the variable indifference point. Nor do they apply to the same phenomenon in other senses, in which the factor of movement is not present.

The fact seems to be rather, that the phenomenon of the indifference point and the so-called positive and negative time errors result from purely central factors. The general law seems to be that in all such estimates we tend to form our judgments around

a mode or central tendency of the series. Toward this mean each judgment tends by virtue of a mental set corresponding to the particular scale or series in question. This is practically the equivalent, for judgment, of Leuba's "Law of Sense Memory"—"There is a natural tendency in us to shift the sensation held in memory towards the middle of the scale of intensities."[23] But our own results seem to indicate that the phenomenon is one of direct perception rather than of memory as such. If it were due, as Wreschner, Leuba and others have supposed, to changes in the memory image during the interval between the standard and the comparison or reproduction the same effect should be present when a given magnitude or intensity is investigated alone—out of relation to a group or series. The present experiments show that this is certainly not the case for extent of movement and probably not for time. Moreover the results of the experiments on reproduction of time of movement indicate that it occurs only when the interval between standard and reproduction is short. This is at least not the most favorable condition for changes in the memory image. Besides, if the illusion is due to such changes, it is not clear why the behavior of the memory image should be so dependent on the general range or group in which the impression happened to occur. It is true that memory images undergo changes, which depend chiefly on the period of their duration. But the phenomenon of the indifference point can not be brought under the law of these changes. The constant errors flanking the indifference point seem rather to be errors of direct perception and their generalization should be a law of perception.

In the case of immediate estimation at least it is not so much a law of memory as a law of judgment, and in the case of immediate reproduction, a correlative law of automatic tendency in performance. It is the operation in judgment of the law of habit and adaptation. Just as a group of diverse and varying movements directed towards a given end gravitate toward an average performance which will economize effort and yet accomplish the end of the activity, so the act of judgment, in the interest of mental economy —and especially the motor process of reproduction, when that method of registration is employed—tends toward an average estimate. It is probable that even when single, unserial stimuli are received or movements made, they are "apperceived" into pretty definite mental sets or sense categories. Thus our movements do not, in their extent and force, form a serial scale or continuum, but fall apart

[23] *Op. cit.*

into rough groups, with rather indefinite limits but with rather definite central tendencies—such groups, for instance, as writing movements, eating movements, dressing movements or various sets of trade and professional movements depending on one's habits and customary occupation. And the constant errors found by Delabarre, Loeb and others when movements in such various directions and of different "category" are compared, are probably due to just these central factors of the judgment of magnitude as much as to any anatomical or physiological facts.

What we have here is somewhat different from the contrast experience. The apparent magnitude of one member is not conditioned so much by its general relation to other members of the series or by the effect of an immediately preceding member as by its rather specific relation to the central tendency or mean or average of the series. It is not the phenomenon of contrast. In fact it is just the reverse, for the law of contrast would tend to make the small magnitude seem still smaller in the presence of the large and the large seem correspondingly greater. Nor can it be classified as a phenomenon of adaptation of attention in which expectation or surprise are supposed to result in constant error of estimation. Adaptation of attention toward the group as a whole would lead to a situation of contrast. The small magnitude would surprise by its unexpected shortness and be underestimated in reproduction, while the constant error for the greater magnitudes would be positive. This again is just the reverse of what we actually find in the present experiment.

This *law of central tendency* may be illustrated in the case of judgments of extent of movement in some such way as the following. Suppose *AC* to represent a scale of magnitudes and *B* to represent a value in the central region, between *A* and *C*. In the attempt, now, to reproduce a given magnitude, every point in the series may be said to exert an attraction on the moving member, by virtue of the automatic character of motor habit—the thing once done tends in the future to carry itself out to completion whenever it is initiated. As a result of this "attraction" the tendency to reproduce a magnitude larger than *AB* is partly inhibited by the tendency to make one less and vice versa. *B* thus becomes the "indifference point," and *AB* the magnitude whose reproduction will be least disastrously affected by the motor habit. In the case of the other senses, though not reenforced by this motor law, the law of *central tendency in judgment* prevails nevertheless to sufficient extent to complicate our measurements and to keep us supplied with "problems."

For the same rule holds, as researches referred to in the first part of the chapter have shown, in cases in which errors of reproduction are not present, cases, *i. e.*, in which the judgment is purely a matter of comparison of sense stimuli—visual lines, visual angles, duration, weight, force, brightness—all show the same phenomenon. The *law of central tendency,* in such cases, produces results analogous to many cases of "preperception." The hunter who mistakes the clump of stubble for a rabbit is the classical example. He is mentally set for "rabbits" and is not engaged in an experiment on sensible discrimination. Hence small differences are disregarded. Anything which roughly approximates the form of a small animal is adequate to provoke the judgment "rabbit." In ordinary life we are not concerned with small differences, we are more occupied with averages, types, central tendencies, general resemblances. It is this fact which frequently permits even a crude counterfeit to pass undetected. Now it appears that this daily habit carries over even into our deliberate experiments on sensible discrimination. Each impression leaves a mental set which tends more or less to assimilate a succeeding impression, just as the set corresponding to or induced by the idea "rabbit" tends to assimilate the clump of stubble. Any stimulus not *too* different is likely to appear identical, even though practised scrutiny with knowledge of results might make the discrepancy apparent. Now it is easy to see why the resulting mental set of the series of magnitudes, light intensities, for example, should correspond to the central tendency of the series. In this region there are, in both directions, magnitudes of general resemblance. In the case of the extreme magnitudes, however, the resemblances run in one direction only. In the central region the stretches or ranges of resemblance overlap and intensify each other, magnitudes within them are mutually taken for each other, and the resultant is a mental set for the central tendency. Every magnitude tends to be assimilated by this set and made to appear less different from the central tendency than it really is. The degree of this assimilation is measured by the amount of difference required to do violence to the mental set in a single instance. Thus, in the case of the hunter, the degree of assimilation is measured by the deviation in contour, shading and general appearance sufficient to provoke a judgment of "not rabbit," in the inexact discriminative processes of a hunter intent on game. Just less than this amount of difference will ordinarily go undetected. Similarly, in the case of light intensities, a rather definite degree of assimilation affects magnitudes on both sides of the

indifference region. This amount of difference is ignored in the process of discrimination. This degree of assimilation is measured by the C.E. This of course will be negative for magnitudes above the region of indifference, since this amount of difference is unnoticed. For the same reason the C.E. will be positive for magnitudes below the region of indifference, *i. e.*, magnitudes at either extreme will tend to appear more nearly equal to the central tendency than they really are. The region of indifference represents the range of magnitudes any of which satisfies the general mental set induced by the series as a whole—they can be roughly substituted for each other without detection.

In this sense the illusions may be said to be due to the effect of expectation, except that in this case expectation results in assimilation instead of in contrast. The words "mental set for the central tendency" simply mean that we are adjusted for or tend to expect the average magnitude, and to assimilate all other magnitudes toward it, to accept them in place of it.

CHAPTER IV

Relation between Extent and Duration

Many hypothetical attempts have been made to simplify the judgment of magnitude in the case of movements by reducing the various perceptions of extent, time, force, speed and position to terms of one or more of these factors. Thus it has frequently been conjectured that the judgment of extent is based on the perception of duration or on the force of contraction, that force is measured in terms of extent or speed, etc. The most frequent suggestion has been that which would make the estimation of extent a function of the perception of time. Loeb[1] was led to this supposition by the observation that some subjects sought to attain greater accuracy by counting during the execution of the movement. But the records of such subjects showed no superiority over those in whom the tendency was not remarked. Kramer and Moskiewicz[2] proposed the same reduction as a result of their experiments on the Loeb illusion already quoted. They supposed that, especially in the presence of such different sensation complexes as were involved in their experiments, the only quality common to the two movements was the element of time and that the durations of these intospectively equal extents would be found to agree. The conjecture, however, was not put to the test. Similarly Jaensch,[3] working on the same problem very recently, finds that times agree much more closely than extents though the errors are not always in the same direction. In fact he finds the time differences to be as small as one could expect even in deliberate attempts to reproduce durations, and concludes that "we hold stretches to be equal the retrospective times of which are equal." Even when the movements were made from the same initial point the times were almost equal and seemed to serve as criteria when visual data were excluded. Külpe, while not adhering to a strict duration theory, says that "as a general rule the apparent magnitude of a distance is proportional to the length of time required for movement across it."

On the other hand we find an array of objections accumulated by Woodworth[4] which seems to discredit completely the foregoing hypotheses. Chief among these objections are the following:

[1] *Pflüger's Arch.*, **41**, 124, 1887.

[2] *Op. cit.*, 125.

[3] *Zeit. f. Psychol.*, **41**, 257–279, 1905.

[4] "Le Mouvement," Ch. IV. "Accuracy of Voluntary Movement," 77 ff.

1. The time of movements may be extremely varied without entirely destroying the approximate equality of their extents.

2. The results obtained by Cattell and Fullerton show that extent can be judged better than time.

3. Although the constant error when one movement is made faster than another is in the direction of compensation it is not sufficient for it.

4. If we judged by time alone the difference between long and short or fast and slow movements would have no meaning for us aside from terms of visual space or of force.

5. There is no *a priori* reason for believing any one perception to be more fundamental than others. The sensations of movement are varied enough to afford each sort of judgment a sensory basis of its own. Introspectively there seems to be no inference from one perception to another.

But Woodworth points out the need of more and crucial experimental data on the precise relation between the two factors and suggests the three following methods of procedure:

1. Confusion of the supposed primitive perception. The other, if derived, should, under such circumstances, be less accurate than ordinarily.

2. The method of correlation or of incidental observation of one factor while the observer is occupied with the other.

3. Separate accuracy tests. No purely derivative perception should be found to be more accurate than the more primitive perception on which it is based. It should, indeed, be less accurate, since the process of derivation would probably introduce additional errors.

The first method here suggested has frequently been applied and has resulted in considerable irregular but incommensurate disturbance of the presumably derived perception. The chief difficulty with the second method seems to have been that of arranging apparatus which would register simultaneously the extent and duration of movements of any considerable magnitude. The method appears not to have been employed until the experiments of Jaensch, and these were under restricted conditions and with unsatisfactory apparatus. The apparatus used in the present research is peculiarly adapted to such procedure, and in the present chapter experiments will be reported in which the method was followed with three observers. The results of Cattell and Fullerton,[5] who found the perception and reproduction of extent to be more accurate than that

[5] "Small Differences," pp. 103–116.

of time have been quoted as an illustration of the third method. But certain disadvantages in the apparatus used in these observations seem to make further experiments desirable. In the experiments reported by these authors the duration of the movement was not completely under the control of the observer. His task was to reproduce, by a movement of 50 cm., the time of a previous movement of the same extent. It was not a question of stopping a controlled movement at the expiration of a given period, but of reaching a certain fixed point at such a time. If the speed was miscalculated no correction could be made, since, if the distant point were not yet reached at the proper time it was necessary to continue the movement beyond the time felt to be sufficient, while, if the point happened to be passed too soon, the record was already made. Under these conditions it would seem that perception of speed rather than perception of duration was under investigation. Moreover these writers report data for only $\frac{1}{4}$, $\frac{1}{2}$ and 1 second. With the duration more perfectly under the control of the observer the perception and reproduction of time might conceivably be found to be more accurate than under the conditions just described. With the present apparatus such conditions are easily fulfilled. As the car moves along the track the duration is recorded accurately and graphically at every point in the progress of the movement. Variations and errors of speed need not interfere with the execution of duration, and the recording of the time is quite independent of the extent passed over. With these favorable conditions experiments on the perception and reproduction of duration were performed on two subjects and these also will be reported in the present chapter. In securing the results to be given later we are thus applying the second and third methods indicated by Woodworth.

On the basis of factor explicitly studied and method followed the experiments may be divided into five groups.

1. Determination of the accuracy of perception and of reproduction of extent. In these experiments four observers were used. On three, W., H. and Bt., the magnitudes ranged from 100 mm. to 400 mm. This total range was broken up into six sections, viz., 100–150, 150–200, 200–250, etc., and 75 trials within each section were made. A trial consisted in (*a*) the execution of a standard movement the magnitude of which was controlled by the signal from the sound hammer; (*b*) the attempt, at the word of the operator, to reproduce the extent of this standard; (*c*) after having completed the reproduction, a guess, indicated by the word "more" or "less," as to the direction in which the error probably lay. With these

three subjects the continuous method was used, the terminal point of the standard serving as the starting point of the reproduction. On subject L. the successive method was employed, the initial points of the two movements being the same. A wider range of magnitudes was thus made possible. The movements used varied between 150 mm. and 650 mm., and the total range has been divided into five sections, viz., 150–250, 250–350, etc., 75 trials being made within each section. In the calculation of error the separate magnitudes were collected under the heading of the section within the limits of which the standards fell and the per cent. error calculated for each movement. These errors were then averaged to secure the average per cent. error for the group, and this error was analyzed into constant and variable errors. By this method it has been possible to avoid the distracting and complicating features involved in earlier methods of controlling the magnitude of the standard movement. There is no illusion of impact and yet the extent is completely under the control of the operator. At a comfortable rate of movement the reaction time in stopping the movement at the signal is exceedingly short. An important advantage lies in the fact that however delayed the reaction may be the space passed over in its execution is included, both introspectively and objectively, in the standard extent and no allowance need be made for it. Not only is the illusion of impact absent but the errors arising when the standards are free movements determined in their extent by the observer himself are not present.[6]

2. Determination of the accuracy of perception and reproduction of duration. These experiments were made on three subjects, W., Bt. and H. The method followed was precisely that of Ex. 1, except that the observer tried to reproduce the duration of the standard movement instead of its extent. In these movements the observer endeavored to move at an approximately constant speed, and this fact enabled the operator to vary the standard duration in the same way in which he varied the extent in the other experiments, by changing the position of the slide, the contact of which with the prong projecting from the car completed the sound hammer circuit and thus gave the signal for stopping the movement. Having executed the standard movement, the observer, at the command of the operator, went on to reproduce its duration by the continuous method. The total range of durations used was between 1 sec. and $3\frac{1}{2}$ sec., and has been divided, for purposes of calculation, into five sections, viz., $1–1\frac{1}{2}$, $1\frac{1}{2}–2$, etc. The calculation of error was performed as in the case of extent: the per cent. error of each trial within a group

[6] See Chapt. II.

being averaged to secure the average error for that section, and this error analyzed into constant and variable errors. Within the limits of each section 75 trials were made. In each case, as in the experiments on extent, the observer, after his attempt at reproduction, guessed as to the probable direction of his error. During the course of the experiment frequent intervals occurred which were below or beyond the range of durations chosen. These were not included in the table.

These two experiments are calculated to bring us one step nearer our conclusion. If we find that the error of perception and reproduction for time is greater than that for extent, as suggested by Cattell and Fullerton, we may be justified in concluding that extent is not judged in terms of time. If the error for time is less than or equal to that for extent, we may still be in some doubt.

3. Incidental observation of the relation between the durations in the experiments in which the observer tried, in his most natural way, to reproduce the extent. Since the instrument records graphically not only the extent but the duration there is no difficulty in securing such a correlation. The observer devotes his attention to the extent of his movements. The observer having made his reproduction and guessed as to the direction of his error, we can determine two facts concerning the process: (*a*) whether the times or the extent of the two movements agree more closely, (*b*) whether or not the subsequent introspective impression indicated by the judgment of "more" or "less" agrees more often with the objective relation of the extents or with that of the times.

Further, the agreement of the durations secured in this way can be compared:

(*a*) With the accuracy of the reproduction of extent. If the times are perceived and reproduced less accurately than the extents we shall not be justified in supposing the perception of time to be the more fundamental of the two. On the other hand, if the times agree more closely than the extents we may be justified in further consideration of the theory.

(*b*) With the accuracy of the perception and reproduction of time intervals when such performance is the explicit and deliberate attempt of the observer. If the agreement of the records incidentally secured is less complete and uniform than in the case of those explicitly performed, we may infer that the judgments of extent were not made in terms of duration. If there is no considerable deviation in the results secured by the two different methods, the theory of the more primitive character of the judgment of time, as proposed by Loeb, Kramer and Moskiewicz and Jaensch, while not demonstrated, is at least made exceedingly plausible.

4. Incidental observation of the relations of the extents in the experiments in which the observer sought to reproduce the times. This procedure is rendered easy by the same features of the apparatus that make the preceding observations possible. In the observer's attempts to reproduce durations we have the extents also recorded, both of the standard and of the reproduction. (*a*) If we find the extents to disagree more than the durations we can draw no conclusion at all except that the perception and reproduction of the time is clearly not a function of the sense of extent of movement. (*b*) But if we find the extents to agree equally with or more closely than the times, it is possible that in reproducing time one seeks to make equal extents at the same speed, and that there is some direct sense for speed aside from the conscious relation of extent and duration.

5. The experiments constituting the fifth group were suggested by the procedure of Cattell and Fullerton in their analysis of the total error into error of perception and error of execution. The objective measurements may be supposed to reveal the basis of the perception involved in the total performance, consisting of the estimation of the standard and the attempt at reproduction. The guesses as to the probable direction of the error we may expect to give certain additional information on the same point. Of course the reproduction is always *intended* to be equal to the standard, and at the final moment the observer feels it to be so. Otherwise he would correct it, if it fell, short, by prolonging it. Or if the second movement happened to "get away" from him and this error was recognized, the consciousness of failure would result in a judgment of "more." *A priori,* then, one might be led to expect all guesses to be "more." As a matter of fact we get guesses of both "more" and "less." If they turn out to be approximately equal in number, no matter what the actual error, we may treat them as merely chance guesses. But if one type considerably exceeds the other we may suppose that its direction was determined by an actual retrospective difference in either the extents or the durations. Thus, if we find guesses of "more" in excess and on consulting the objective relations of the movements we find a tendency to a positive constant error in the times while the errors of the extents are indifferently distributed or are negative, we would not be unjustified in assuming that the direction of the guesses was determined by the perception of time rather than by that of extent. Further inference would depend on the nature of the task which the observer was explicitly trying to accomplish in the trials concerned.

Certain other points descriptive of the experiment as a whole

may now be indicated. In all trials visual criteria were eliminated, either by having the observer work with closed eyes or by allowing him to wear a blind mask. In the beginning of the experiment the ears were plugged in order to exclude the secondary criteria afforded by the noise of the apparatus or by the sound of the vibrating reed interrupter which controlled the magnet of the time marker. But very early in the experiment improvements in the running gear of the car made the operation of this apparatus noiseless, and at the same time the interrupter was muffled by the use of boxing and padding. This was probably an unnecessary precaution, however, for the vibrations at the rate of 10 per second can scarcely serve as criteria for duration.

TABLE XI

REPRODUCTION OF EXTENT. FIRST COLUMN, DELIBERATE. SECOND COLUMN, INCIDENTAL

Observer W.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	8	12	12	18	14	15	9	18	13	13	16	19	12	16
	C.E.	− 1	+ 1	+ 9	+ 5	+16	−10	0	−11	− 1	−10	−12	−19	− 2	− 7
	V.E.	*8*	*11*	*10*	*19*	*14*	*13*	*9*	*15*	*13*	*12*	*11*	*7*	*11*	*13*
	Trials	15	25	15	3	15	13	15	22	15	15	15	7	90	85
5	A.E.	15	11	14	10	23	14	18	5	19	9	12	14	17	11
	C.E.	+ 1	+ 6	+ 5	+ 5	+23	− 2	+ 5	+ 1	− 4	− 7	+ 1	−14	+ 5	− 2
	V.E	*15*	*10*	*14*	*9*	*16*	*13*	*17*	*7*	*18*	*6*	*11*	*7*	*15*	*9*
	Trials	15	19	15	10	15	15	15	14	15	16	15	8	90	82
10	A.E.	13	35	20	19	22	23	13	12	12	13	17	20	16	20
	C.E.	− 2	+ 32	+10	+10	+13	+13	+ 5	+ 2	− 1	− 7	−15	−20	+ 2	+ 4
	V.E.	*12*	*22*	*19*	*18*	*20*	*22*	*11*	*11*	*12*	*10*	*10*	*6*	*14*	*15*
	Trials	15	15	15	25	15	13	15	15	15	17	15	11	90	96
15	A.E.	20	19	10	12	15	16	15	17	16	11	12	12	15	16
	C.E.	+12	+ 11	+ 4	+ 1	+ 7	+ 4	+ 3	+ 6	− 2	− 1	− 8	−12	+ 3	+ 2
	V.E.	*19*	*18*	*10*	*11*	*14*	*14*	*11*	*14*	*16*	*11*	*11*	*5*	*14*	*12*
	Trials	15	21	15	17	15	19	15	14	15	11	15	4	90	86
30	A.E.	26	18	20	14	16	21	23	18	25	17	47	33	28	20
	C.E.	−25	+ 2	−17	− 4	−26	−14	−23	−12	−25	−12	−47	−33	− 27	− 12
	V.E.	*14*	*19*	*17*	*13*	*9*	*14*	*7*	*17*	*10*	*14*	*4*	*8*	*10*	*14*
	Trials	15	20	15	17	15	18	15	18	15	13	15	2	90	88
Av.	A.E.	16	19	15	15	20	18	16	14	17	13	21	20	18	17
	C.E.	− 3	+ 10	+ 2	+ 3	+ 7	− 2	− 2	− 4	− 7	− 7	−16	−20	6	8
	V.E.	*13*	*16*	*14*	*14*	*15*	*15*	*11*	*13*	*14*	*11*	*9*	*7*	*13*	*13*
	Trials	75	100	75	72	75	78	75	83	75	72	75	32	450	437

In order to secure information concerning the influence, on the accuracy of reproduction, of the time interval elapsing between the standard and the attempt to repeat it, five different intervals were

used throughout all the experiments, 15 trials for each section, both for time and for extent, being made with intervals of 2 sec., 15 with intervals of 5 sec., and similarly with intervals of 10 sec., 15 sec. and 30 sec., these constituting, for each section, the 75 trials mentioned above. The results on the influence of the time interval will be brought together in Chapter VI.

TABLE XII

Reproduction of Extent. First Column, Deliberate. Second Column, Incidental

Observer H.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	25	25	19	12	19		16	15	13	12	10	9	17	15
	C.E.	+25	+25	+19	+ 9	+19		+13	+ 5	+10	— 5	+ 8	— 6	+16	+16
	V.E.	*14*	*13*	*10*	*9*	*9*		*10*	*14*	*11*	*11*	*10*	*7*	*11*	*11*
	Trials	15	12	15	9	15		15	11	15	14	15	23	90	69
5	A.E.	33	21	20	18	25	24	23	15	10	11	11	10	20	17
	C.E.	+33	+11	+20	+15	+22	+10	+23	+15	+ 7	+ 4	+ 2	+ 1	+18	+ 9
	V.E.	*14*	*24*	*10*	*13*	*13*	*22*	*11*	*8*	*8*	*10*	*11*	*10*	*11*	*15*
	Trials	15	15	15	11	15	11	15	16	15	18	15	14	90	85
10	A.E.	38	15	22	34	22	22	29	17	15	14	9	11	23	19
	C.E.	+38	+10	+22	+30	+22	+12	+29	+13	+14	+14	+ 6	— 7	+22	+11
	V.E.	*11*	*9*	*14*	*20*	*9*	*17*	*9*	*10*	*8*	*12*	*7*	*10*	*10*	*15*
	Trials	15	4	15	5	15	9	15	23	15	24	15	20	90	85
15	A.E.	25		28	20	27	15	24	13	16	12	20	12	23	14
	C.E.	+25		+25	+19	+27	+11	+24	+ 4	+13	— 9	+20	—12	+22	+ 3
	V.E.	*17*		*15*	*9*	*12*	*10*	*9*	*14*	*12*	*10*	*7*	*7*	*12*	*10*
	Trials	15		15	11	15	23	15	15	15	13	15	11	90	73
30	A.E.	18	12	30	5	21	12	29	11	21	10	9	7	*21*	*10*
	C.E.	+ 7	— 7	+28	+ 1	+21	— 1	+29	+10	+16	+ 3	+ 4	— 1	+18	+ 1
	V.E.	*16*	*8*	*19*	*5*	*18*	*12*	*17*	*8*	*14*	*10*	*9*	*7*	*16*	*8*
	Trials	15	5	15	3	15	13	15	21	15	26	15	16	90	84
Av.	A.E.	28	18	24	18	23	18	24	14	15	12	12	10	21	15
	C.E.	+26	+10	+23	+14	+22	+ 8	+24	+ 9	+12	+ 7	+ 8	— 5	19	9
	V.E.	*14*	*16*	*14*	*11*	*12*	*15*	*11*	*11*	*11*	*11*	*9*	*8*	*12*	*12*
	Trials	75	36	75	39	75	56	75	86	75	95	75	84	450	396

So far as possible the experiments were performed in an order which would distribute the results of practise. All the subjects had made a great many preliminary trials before the records presented in the present chapter were taken. In general but one or two time intervals were used at a single sitting, and the various magnitudes were taken in a chance order, except toward the end of the experiment, when it became necessary to give definitely chosen magnitudes in order to secure the requisite number of trials for each section of the total range.

The results for each observer, for both extent and duration, are shown in full in Tables XI. to XVII. The records on separate accuracy are given in each table along with the errors for the same factor, for W., H. and Bt. when the observer's attention is fixed on the reproduction of the other factor. Thus in Table XI. are given the A.E., C.E. and V.E. for observer W., for the perception and reproduction of extents—in the first column under each section

TABLE XIII

Reproduction of Extent. First Column, Deliberate. Second Column, Incidental

Observer Bt.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	25	31	17	23	22	9	16	26	12	20	22	13	19	20
	C.E.	+23	+30	+10	+19	+ 8	— 3	— 2	—10	— 5	—20	—21	—13	+ 2	+ 1
	V.E.	*14*	*16*	*17*	*18*	*22*	*13*	*16*	*23*	*12*	*6*	*9*	*9*	*15*	*14*
	Trials	12	12	11	14	10	10	10	11	10	6	10	10	63	63
5	A.E.	26	26	27	30	8	16	16	13	19	28	27	17	21	22
	C.E.	+23	+22	+25	+27	+ 6	—16	+13	+ 3	—17	—16	—27	—17	+ 4	0
	V.E.	*23*	*19*	*21*	*19*	*6*	*9*	*15*	*12*	*12*	*15*	*11*	*9*	*15*	*14*
	Trials	11	16	10	10	10	5	10	6	13	5	10	11	63	53
10	A.E.	58	41	35	14	32	16	10	17	14	21	23		29	22
	C.E.	+51	+37	+31	+10	+32	+ 9	— 4	—16	—10	—20	—23		+13	+ 4
	V.E.	*38*	*26*	*21*	*10*	*17*	*15*	*13*	*7*	*13*	*9*	*10*		*19*	*13*
	Trials	11	18	10	6	10	11	14	8	13	11	10		68	54
15	A.E.	87	45	56	27	28	16	15	15	13	22	25	26	37	25
	C.E.	+87	+41	+56	+ 4	+26	— 2	+ 8	— 9	— 4	—11	—25	—26	+25	— 1
	V.E.	*32*	*35*	*29*	*26*	*17*	*16*	*13*	*8*	*14*	*18*	*12*	*6*	*20*	*18*
	Trials	10	10	10	11	10	4	10	15	9	8	10	10	59	58
30	A.E.	73	27	50	37	30	18	17	16	13	21	30	32	36	25
	C.E.	+73	+21	+49	+37	+27	+ 7	+ 9	+16	— 1	+7	—30	—32	+21	+ 9
	V.E.	*42*	*25*	*40*	*26*	*27*	*17*	*15*	*10*	*13*	*21*	*7*	*7*	*26*	*18*
	Trials	5	10	5	6	5	8	5	6	6	8	8	12	33	50
Av.	A.E.	54	34	37	26	24	15	15	17	14	22	25	22	28	23
	C.E.	+51	+30	+34	+14	+20	— 1	+ 5	— 3	— 7	—12	—25	—22	24	15
	V.E.	*29*	*24*	*26*	*20*	*18*	*14*	*14*	*12*	*13*	*14*	*10*	*8*	*18*	*15*
	Trials	48	66	46	47	45	38	49	46	51	38	48	43	287	264

when the attempt is to reproduce extent, in the adjacent column when the attempt is to reproduce the durations, the extent being recorded incidentally. In the case of observer L. only trials for the reproduction of extent were made, and the table gives, alongside these errors, the corresponding errors for the durations of the same movements. In all the tables the records are grouped under their appropriate sections, and the results for the different time intervals are indicated separately. At the foot of each table the results for the

particular sections for all five time intervals are averaged, while on the right of each table are brought together the results of each section for the particular time intervals. Reading across at the foot of the table from left to right we have given the influence of the magnitude on the amount of the error, while reading down the

TABLE XIV

Reproduction of Duration. First Column, Deliberate. Second Column, Incidental

Observer W.

Interval	Error	1–1.5 sec.		1.5–2 sec.		2–2.5 sec.		2.5–3 sec.		3–3.5 sec.		* 3.5–4 sec.	1–3.5 sec. Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	Per Cent.	
2	A.E.	8	22	10	15	11	16	11	18	11	28	48	10	20
	C.E.	− 1	+14	+ 1	+ 4	0	− 1	− 6	− 4	− 7	−28	−48	− 2	− 3
	V.E.	*8*	*21*	*10*	*14*	*14*	*16*	*10*	*16*	*9*	*4*	*17*	*10*	*14*
	Trials	15	24	15	18	15	28	15	13	15	6	8	75	89
5	A.E.	15	34	18	22	13	19	8	19	8	27	22	13	24
	C.E.	+12	+23	+ 3	+11	+ 2	− 7	− 0.2	− 9	− 4	−24	+ 7	+ 3	− 1
	V.E.	*10*	*28*	*18*	*18*	*17*	*18*	*8*	*17*	*7*	*14*	*22*	*12*	*19*
	Trials	15	18	15	12	15	24	15	23	15	11	9	75	88
10	A.E.	26	35	24	19	12	21	8	20	7	16	20	16	22
	C.E.	+26	+29	+24	− 1	+ 4	+15	+ 2	+ 5	+ 2	+ 2	−14	+12	+10
	V.E.	*11*	*29*	*11*	*19*	*12*	*20*	*7*	*19*	*9*	*16*	*16*	*10*	*21*
	Trials	15	8	15	20	15	15	15	18	15	15	13	75	76
15	A.E.	27	29	13	24	10	36	11	22	12	18	26	15	26
	C.E.	+23	+20	+ 9	+17	+ 4	+21	+ 4	−22	+ 9	− 1	−21	+10	+ 7
	V.E.	*17*	*30*	*12*	*19*	*9*	*35*	*12*	*18*	*10*	*18*	*17*	*12*	*24*
	Trials	15	8	15	8	15	11	15	5	15	17	18	75	49
30	A.E.	18	29	13	25	8	18	7	17	10	29	19	11	24
	C.E.	+ 7	+ 8	+ 0.2	+13	− 1	+12	+ 7	+ 5	+ 3	+13	− 5	+ 4	+10
	V.E.	*16*	*26*	*13*	*22*	*8*	*17*	*5*	*12*	*10*	*27*	*19*	*10*	*21*
	Trials	15	3	15	10	15	10	15	3	15	15	14	75	41
Av.	A.E.	19	30	16	21	11	22	9	19	10	23	27	13	23
	C.E.	+14	+15	+ 8	+ 9	+ 2	+ 8	+ 1	+ 5	+ 0.4	−14	−15	5	10
	V.E.	*15*	*27*	*13*	*18*	*11*	*21*	*8*	*16*	*9*	*16*	*18*	*11*	*20*
	Trials	75	61	75	68	75	88	75	62	75	64	62	375	343

* Incidental.

column on the right the figures indicate the influence of the lengthening of the time interval on the accuracy for the total range of the standard magnitudes. The figures in the lower right hand corner of each table represent the average errors of the total range regardless of the difference in time interval. We are now ready to analyze the results according to the principles outlined above.

Taking up the five experiments in the order in which they have been described, we have first to consider those on the accuracy of perception and reproduction of extent of movement when the stand-

ard magnitudes are neither free movements nor movements terminating in impact, but controlled movements, the magnitudes of which are determined by their arrest on the part of the subject at the sound hammer signal. The results for the four observers are to be found in the first columns under each section in Tables XI., XII., XIII. and XVII.

TABLE XV

REPRODUCTION OF DURATION. FIRST COLUMN, DELIBERATE. SECOND COLUMN, INCIDENTAL

Observer H.

Int.	Error	1–1.5 sec.		1.5–2 sec.		2–2.5 sec.		2.5–3 sec.		3–3.5 sec.		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	23	31	15	15	9	9	7	11	7	12	12	16
	C.E.	+23	+31	+11	+15	+ 5	+ 2	+ 5	+ 2	− 2	−12	+ 9	+ 8
	V.E.	*6*	*10*	*12*	*13*	*8*	*9*	*6*	*16*	*6*	*7*	*8*	*11*
	Trials	15	18	15	34	15	26	15	9	15	3	*15*	90
5	A.E.	26	22	20	15	26	12	15	9	14	10	20	14
	C.E.	+26	+22	+19	+14	+10	+ 2	+15	− 3	+12	+ 5	+17	+ 8
	V.E.	*9*	*12*	*10*	*10*	*11*	*12*	*8*	*8*	*10*	*7*	*9*	*10*
	Trials	15	23	15	30	15	19	15	8	15	7	75	87
10	A.E.	14	22	26	14	29	14	23	9	22	17	23	15
	C.E.	+ 5	+15	+26	+10	+29	+ 8	+23	+ 9	+20	+ 8	+20	+10
	V.E.	*14*	*19*	*17*	*13*	*17*	*12*	*8*	*12*	*11*	*15*	*13*	*14*
	Trials	15	17	15	22	15	29	15	7	15	5	75	80
15	A.E.	19	23	23	19	11	14	8	9	31		19	13
	C.E.	+13	+18	+23	+16	+ 8	+ 8	+ 7	− 4	+29		+16	+ 9
	V.E.	*15*	*18*	*13*	*15*	*12*	*12*	*5*	*9*	*18*		*12*	*12*
	Trials	15	23	15	30	15	26	15	6	15		75	85
30	A.E.	33	16	26	17	16	16	17	14	33		25	13
	C.E.	+33	+13	+26	+13	+10	+13	+ 8	− 2	+30		+21	+ 9
	V.E.	*23*	*13*	*11*	*14*	*14*	*15*	*15*	*13*	*18*		*16*	*14*
	Trials	15	15	15	24	15	16	15	8	15		75	63
Av.	A.E.	23	23	22	16	18	13	14	11	22	13	20	15
	C.E.	+20	+20	+21	+14	+12	+ 7	+12	+ 1	+16	+ 0.3	16	8
	V.E.	*13*	*14*	*12*	*13*	*12*	*12*	*8*	*12*	*13*	*10*	*12*	*12*
	Trials	75	96	75	140	75	116	75	38	75	15	375	405

For W. the gross average per cent. error for the several sections varies between 15 per cent. and 21 per cent., with an average, for the total range, of 18 per cent., and increases with the magnitudes in rather close approximation to Weber's law, the D.L. being between $\frac{1}{6}$ and $\frac{1}{5}$. The C.E.'s are not large in the final average, though they range from — 16 per cent. to + 7 per cent. in the separate sections, the average regardless of signs,[7] for the total range being 6 per cent.

[7] In calculating the final average C.E.'s signs have been disregarded in all these tables, since it is the absolute magnitude rather than the direction of these errors which is significant as a measure of accuracy.

The phenomenon of the indifference point is found here, but is seen to be due to the constant errors in the cases in which the intervals were rather short, no positive errors occurring in the individual averages beyond the interval of 15 sec. With these short intervals

TABLE XVI

REPRODUCTION OF DURATION. FIRST COLUMN, DELIBERATE. SECOND COLUMN, INCIDENTAL

Observer Bt.

Interval	Error	1–1.5 sec.		1.5–2 sec.		2–2.5 sec.		2.5–3 sec.		3–3.5 sec.		Averages and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	31	23	26	21	11	16	22	23	20	14	22	19
	C.E.	+29	+19	+26	− 4	0	−10	−20	−20	−20	−14	+ 3	− 6
	V.E.	*17*	*19*	*9*	*21*	*11*	*16*	*13*	*20*	*9*	*10*	*12*	*17*
	Trials	10	18	10	10	10	15	10	5	10	10	50	58
5	A.E.	30	34	26	18	15	24	20	26	10	30	20	26
	C.E.	+20	+25	+24	+ 6	+ 3	+11	+ 2	−15	− 6	−30	+ 9	− 1
	V.E.	*19*	*33*	*16*	*22*	*15*	*21*	*21*	*24*	*10*	*9*	*16*	*22*
	Trials	10	13	10	12	16	20	10	14	10	11	56	70
10	A.E.	52	42	18	41	36	21	16	21	18	32	28	31
	C.E.	+52	+40	+17	+18	+36	− 3	+11	−20	− 3	−32	+22	− 1
	V.E.	*24*	*29*	*17*	*37*	*7*	*20*	*15*	*18*	*18*	*7*	*16*	*22*
	Trials	12	18	10	16	10	15	10	8	10	5	52	62
15	A.E.	51	37	32	17	18	24	13	21	12	32	25	26
	C.E.	+51	+35	+27	+13	+12	−14	+ 1	+ 4	− 9	−32	+16	+ 1
	V.E.	*30*	*22*	*17*	*13*	*13*	*20*	*13*	*21*	*10*	*15*	*17*	*18*
	Trials	16	24	10	19	10	13	10	4	10	2	56	62
30	A.E.	48	75	62	24	37	26	15	18	18	23	36	34
	C.E.	+48	+75	+62	+24	+37	+ 6	− 3	− 6	−18	− 5	+25	+19
	V.E.	*25*	*26*	*29*	*14*	*12*	*24*	*15*	*20*	*6*	*23*	*17*	*21*
	Trials	10	7	10	9	10	10	10	7	10	2	50	35
Averages.	A.E.	42	40	33	24	23	22	17	22	16	26	26	27
	C.E.	+40	+39	+31	+11	+18	− 2	− 2	−11	−11	−23	20	17
	V.E.	*23*	*26*	*18*	*21*	*12*	*20*	*15*	*21*	*11*	*13*	*16*	*20*
	Trials	58	80	50	66	56	73	50	38	50	30	264	287

the effect on judgment of the total range of magnitudes has time to operate. In the case of the longer intervals the separate trials are distributed over so great a period that the group influence is not found to be effective, the C.E.'s being all negative. The final variable errors range from 9 per cent. to 15 per cent., averaging 13 per cent., and increase in rough accord with Weber's law, though there is indication, in the fourth and sixth sections, of a tendency toward a slower rate of increase.

Turning to H.'s table we find remarkable similarity in the results. The A.E.'s tend to be slightly larger than for W., ranging from 12 per cent. to 28 per cent., with a final average of 21 per cent. as over against 18 per cent. for W. The V.E.'s are identical for

the second, fourth and sixth sections, and in no section is the difference more than 3 per cent. The final average V.E. for H. is 12 per cent., as over against 13 per cent. for W. Only in the case of the C.E.'s is there great deviation. H. has no negative C.E.'s, hence no indifference point occurs. It may be noted that this observer is the writer and that he had already performed the experi-

TABLE XVII

Reproduction of Extent. First Column, Extents. Second Column, Duration

Observer L.

Int.	Error	mm. 150-250		mm. 250–350		mm. 350–450		mm. 450–550		mm. 550–650		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	16	26	9	19	12	17	5	14	5	13	11	18
	C.E.	−12	+15	− 6	+10	−10	− 2	+ 0.3	− 8	− 2	−13	− 6	+ 0.6
	V.E.	*11*	*20*	*7*	*15*	*5*	*17*	*5*	*10*	*4*	*6*	*6*	*14*
	Trials	15		15		15		15		15		75	
5	A.E.	13	16	8	13	13	14	9	13	6	15	9	14]
	C.E.	− 1	+ 8	− 6	+ 1	−13	−10	− 8	−12	− 3	− 9	− 6	− 4
	V.E.	*13*	*17*	*6*	*13*	*5*	*10*	*6*	*7*	*5*	*10*	*7*	*11*
	Trials	15		15		15		15		15		75	
10	A.E.	13	12	12	12	12	19	12	19	9	20	12	17
	C.E.	−10	+ 4	− 9	−11	−11	−13	−10	−13	− 9	−20	−10	−10
	V.E.	*7*	*14*	*10*	*10*	*10*	*14*	*9*	*9*	*6*	*6*	*8*	*11*
	Trials	15		15		15		15		15		75	
15	A.E.	14	24	11	6	6	21	5	18	3	17	8	20
	C.E.	− 5	− 8	− 8	− 3	− 3	− 9	− 3	−14	− 0.5	−14	− 4	−11
	V.E.	*14*	*20*	*9*	*6*	*6*	*16*	*5*	*12*	*3*	*14*	*8*	*16*
	Trials	15		15		15		15		15		75	
30	A.E.	14	20	7	16	16	18	9	14	8	26	12	18
	C.E.	− 7	+ 5	+ 1	−13	−13	− 9	− 9	−14	− 6	−14	− 7	− 7
	V.E.	*11*	*19*	*7*	*11*	*11*	*17*	*5*	*7*	*5*	*20*	*7*	*15*
	Trials	15		15		15		15		15		75	
Av.	A.E.	14	20	9	12	12	18	8	16	6	18	10	17
	C.E.	− 7	+ 5	− 6	−10	−10	− 9	− 6	−13	− 4	−14	7	5
	V.E.	*11*	*18*	*8*	*7*	*7*	*15*	*6*	*9*	*5*	*11*	*7*	*13*
	Trials	75		75		75		75		75		375	

ments of Chapter IV. The knowledge of the usual tendency to constant errors in such trials seems to have operated, though quite unintentionally, toward correction. The C.E.'s range between + 8 per cent. and + 26 per cent., averaging 19 per cent. as against 6 per cent. for W. With this observer all the errors increase considerably more slowly than Weber's law would require, and somewhat more rapidly than the square root law of Cattell and Fullerton suggests.

The errors in the case of Bt. are still larger. The A.E's ranging from 14 per cent. to 54 per cent., averaging 28 per cent., the C.E's

ranging through — 25 per cent. to + 51 per cent., averaging 24 per cent. and the V.E's varying between 10 per cent. and 29 per cent., and increasing much more slowly than the square root law. The final average V.E. for Bt. is 18 per cent., as against 13 per cent. for W. and 12 per cent. for H. The phenomenon of the I.P. shows itself after all intervals.

The results secured from observer L., by the successive method, appear in Table XVII., in the first column under each section and show slightly greater accuracy of reproduction than was found in the preceding cases. The size of the final averages, however, is likely to be misleading here. The A.E. and V.E. increase with the magnitude at a rate approximating the square root law, and since the upper limit of the range of standard magnitudes is much higher here than in the case of W. and H. the final average errors are expressed, in per cents, by smaller figures. But if we inspect the lower part of the range which is common to all the tables, the errors are found to be only slightly less. For L. the A.E.'s for the total range vary between 6 per cent. and 14 per cent., averaging 10 per cent., the C.E.'s are all negative and range from — 4 per cent. to — 10 per cent., averaging 7 per cent., while the V.E.'s, lying between 5 per cent. and 11 per cent., average 7 per cent. The absence of an indifference point is to be explained by the fact that no effort was made to produce a series effect—that is to say, the magnitudes used at a single sitting or taken on a single record sheet did not vary over the total range but lay within a few adjacent sections.

The results of the second group of experiments, on perception and reproduction of duration, are indicated in Tables XIV.–XVI. in the first columns under each section. For W. the A.E.'s for the separate sections range from 9 per cent. to 19 per cent., averaging 13 per cent., and increasing with the magnitude of the standard in close agreement with a cube root law. The sectional C.E.'s range from + 1 per cent. to + 14 per cent., averaging 5 per cent. Of considerable interest is the fact, already mentioned in another connection, that an indifference point shows itself only when the time intervals are so short as to allow the judgment to be influenced by the group effect. The variable error ranges through 8–15 per cent., averaging 11 per cent. and increasing with the magnitude with an approximation to a cube root law. In the table for H. the A.E.'s increase according to Weber's law, ranging between 14 per cent. and 23 per cent. and averaging 20 per cent. The C.E.'s are positive, varying from 12 per cent. to 21 per cent. with an average at 16 per cent. No indication of an indifference point is present except after the 2-sec. interval. The V.E. seems to follow Weber's

law, being remarkably constant in terms of per cent. and averaging 12 per cent. for the total range as over against 11 per cent. in the case of W.

In the case of Bt. again, all errors are greater, and the constant errors change their sign at the indifference point for this particular range of magnitudes—about 2.5 sec. All errors increase too slowly for even a square root law, the final averages being, A.E. 26 per cent., C.E. 20 per cent., V.E. 16 per cent.

These two experiments completed, we have complied with the conditions of the third method proposed by Woodworth. We now have separate accuracy tests for extent and for such durations as are ordinarily employed in the execution of such extents, and are in position to compare the records. Bringing together the final averages for the three subjects we have the following.

TABLE XVIII[8]

	W.			H.			Bt.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent	18	6±2	*13±.6*	21	19±1.7	*12±.6*	28	24±3.8	*18±1.5*
Time	13	5±1.3	*11±.7*	20	16±2	*12±.9*	26	17±2.8	*16±1.2*

If these averages are taken at their face value the results are just the reverse of those of Fullerton and Cattell. With the single exception of H.'s V.E. all the actually obtained errors are less for time than for extent. But the differences in the case of the V.E.'s of W. and Bt. are exceedingly small, and in the case of Bt. quite within the limits of error of the averages compared. The same is true of the C.E.'s of W. and H., while H.'s V.E.'s agree. Positive inference as to the greater accuracy of perception and reproduction of time would be insecure, although the results tend on the whole, to suggest such an inference. The separate accuracy tests, then, yield little information as to the probable basis of the judgment of extent. If the errors in the case of extent had been smaller than those for time there would have been reasonable certainty that the

[8] In Tables XVIII., XIX. and XX. the reliability of the average V.E. and C.E. is given in terms of mean square error. The measure of variability was calculated from the formula $(\text{A.D.} \times 1.25)/\sqrt{n}$, in which A.D. = the average deviation of separate V.E.'s of Tables XI.–XVI. from their final averages, and n = the number of determinations of the V.E. under different conditions of magnitude and interval. In most cases this number was 30. The chances are thus about 2:1 (more exactly, 68:32) that the true final average does not differ from that obtained from our figures by more than this mean square error. Thus in Table XVIII. the chances are 2:1 that W.'s V.E. for extent lies between 12.4 and 13.6, or similarly that his V.E. for time is not less than 10.3 or greater than 11.7. See Thorndike, "Mental and Social Measurements," Science Press, New York, 1904, pp. 59 and 139.

judgment of extent was not made on the basis of the duration of the movement. On the other hand, had the accuracy for time been greater than that for extent, there would have been reason for supposing the temporal judgment to be the more fundamental. Perhaps as reasonable a conclusion as any from the figures as they stand would be that the judgments of the time and the extent of our movements are identical.

This can be decided finally only by the methods of correlation next to be considered. Of the three methods proposed by Woodworth the first and third yield results somewhat favorable to the hypothesis, through the following facts:

(*a*) Disturbance of the time element has been found, by other investigators, to confuse the perception of extent.

(*b*) The present experiment seems to suggest the possibility of slightly greater accuracy of perception and reproduction of time, though the evidence is slight. But the experiment affords positive evidence that the accuracy for time is not less than for extent.

We have now to consider the agreement of the durations when the observer is attending to the extents, in the cases of all four subjects. The records for W., H. and Bt. are to be found in Tables XIV.–XVI., in the second columns under each section heading, parallel with the errors when the durations themselves are the object of attention. For W. the gross errors, ranging from 19 per cent. to 30 per cent., average 23 per cent., the final C.E. is 10 per cent. and the V.E.'s, varying between 16 per cent. and 27 per cent., average 20 per cent. For H. the A.E., C.E. and V.E. are considerably lower, averaging 15 per cent., 8 per cent. and 12 per cent. For Bt. the gross errors range through 22 per cent. to 40 per cent., averaging 27 per cent., the final C.E. is 17 per cent. and the V.E.'s, varying between 13 per cent. and 26 per cent., average 20 per cent. The results for L. are indicated in Table XVII., parallel with his records for extent. In the calculation of these results a slightly different method was used. The durations did not range widely, since there seemed to be a tendency to make all the movements in a time between the limits of .8 and 1.5 sec.[9] Consequently, in calculation the durations were not distributed under sections but the actual durations employed in making movements of a given extent section were compared and the error computed for the durations under each extent section. In the two columns then we have the error for extent and the error for duration for the same movements. The

[9] This tendency may stand in some relation to the idea of a most comfortable interval advanced by Jaensch in the case of movements, and by others in general studies of time sense.

A.E. for duration is 17 per cent., the C.E. 5 per cent and the V.E. 13 per cent. We are now in position to compare the accuracy for extent with the agreement of the durations incidentally secured. We may bring together the results as follows:

TABLE XIX

	W.			H.			Bt.			L.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent	18	6±2	13±.6	21	19±1.7	12±.6	28	24±3.8	18±1.5	10	7± .8	7±.6
Time	23	10±1.8	20±.9	15	8± .9	12±.9	27	17±2.8	20±1.3	17	5±1.5	13±.9

Two features of this table seem to me to afford basis for positive inference of considerable reliability. First, if we take the V.E.'s alone, we get results decidedly the reverse of those of Table XVIII. In that table the errors for extent were never smaller than for time. In the present table they are never larger. On the contrary, the V.E. for time is greater by 50 per cent. in the case of W., by 100 per cent. in the case of L. With Bt. the V.E. for time is greater, but the probable error is larger here than with the other subjects. In H.'s case the V.E.'s for extent and time under all conditions average exactly the same and have about the same probable error. It should be noted that H. is the writer and that in all the observations on him he was aware of the procedure and purpose of the experiment. This fact was felt introspectively to give a certain prominence to temporal relations even in reproductions of extent, while in reproductions of duration attention was called to spatial relations more than would be the case with a naive subject. Bt. also knew something of the purpose of the experiment, having listened to a short preliminary report. The rather close agreement in the case of these two subjects, in contrast with the striking differences in the case of the other two, is probably indicative of the difference in attitude resulting from the knowledge of the experiment and the unnatural prominence given to the incidental factor in each judgment. Comparing Tables XVIII. and XIX. we may say, with considerable certainty, that, although deliberate times fall out somewhat more accurately than extents, incidental times are subject to greater V.E.'s than their corresponding extents. There must, therefore, be some regulation of the extent independent of the incidental regulation of duration.

We have yet to observe the C.E.'s of Table XIX. In the case of W. the C.E. for time is greater than that for extent, agreeing with the relation of the V.E.'s and confirming the results of the preceding paragraph. In the other three cases the C.E.'s are smaller for time. In the case of these three observers the C.E.'s throughout

the experiment are smaller for the incidental magnitudes than for the deliberate (for extent, see Table XX.). With these three subjects the C.E. seems to be more intimately bound up with the process of deliberate reproduction, or of attention to a certain magnitude. When extent is attended to it is subject to a greater C.E. than when its reproduction is incidental to the reproduction of a time magnitude. The C.E for this deliberately reproduced extent is, moreover, greater than that of the time magnitudes incidentally employed. Similarly the deliberate reproduction of time is affected with greater C.E. than the incidental reproduction of either time or extent. Although this relation does not hold for subject W., its prominence in the results of H., Bt. and L. seems to indicate a certain separation between the magnitude attended to and the other. This separation would argue for separate processes of judgment for the two magnitudes, extent and time.

We have yet to compare our incidental measurements of duration with those yielded by the deliberate attempt to reproduce time, in the case of observers W., H. and Bt. These records are to be found in Table XX., along with the corresponding records for extent.

TABLE XX

	W.			H.			Bt.			L.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent—												
deliberate	18	6±2	13±.6	21	19±1.7	12±.6	28	24±3.8	18±1.5	10	7± .8	7±.6
incidental	17	8±1.7	13±.8	15	9±1.3	12±.6	23	15±2.2	15±1.2			
Time—												
deliberate	13	5±1.3	11±.7	20	16±2	12±.9	26	20±3.5	16±1.2			
incidental	23	10±1.8	20±.9	15	8± .9	12±.6	27	17±2.8	20±1.3	17	5±1.5	13±.9

In the earlier description of this experiment the criterion for our conclusion was stated in these words: "If there is no considerable deviation in the results secured by the two methods (deliberate and incidental), the theory of the more primitive character of the judgment of time . . . is at least made exceedingly plausible." But "considerable deviations" are present. Only in the case of H. is there approach to agreement, the V.E.'s here being equal. The significance of the smaller C.E. for incidental time, shown here in the case of H., Bt. and L., has already been discussed. But the V.E.'s for W. and Bt. are much greater for incidental than for deliberate time, almost twice as large for W. and a fourth again as large for Bt. If duration were used as the basis of the judgment of extent, we should expect as great accuracy in the case of the incidental times as in the case of the deliberate. Except for the observation on the writer, this is not found.

Summing up the results of the experiment up to this point, we may say that though the argument is not overwhelming, the balance of evidence seems to show that while deliberate times are somewhat more accurately reproduced than deliberate extents, incidental times show an error greater than either, and this fact, along with the character of the C.E.'s points to separate processes of judgment for the two magnitudes. There is at least no justification for the attribution of more fundamental character to either judgment.

There is still another method of handling the data afforded by these experiments which may be supposed to throw some light on the amount of interdependence between the judgments of extent and of duration—the method of correlation. Ignoring the actual magnitude of error, we may regard each particular error solely from the point of view of its direction and correlate the errors for extent, in the case of a standard and its reproduction, with the incidental error or difference in the corresponding durations. Similar correlations may be made between the errors for time (in the attempt to reproduce duration) and the incidental differences of the corresponding extents. Such correlations have been made, for both cases, and for all observers, by the method of unlike signs. Thus each error in the case of reproduction of extent was classified as + or —, and the errors of the incidental times classified in the same way, regardless of their magnitude. These signs were then compared, in the case of each separate trial, for each magnitude and the per cent. of unlike signs computed for each observer. The same calculation was made for the reproductions of duration. From the resulting per cent. of unlike signs we are able to get the approximate coefficients of correlation given in Table XXI.

TABLE XXI

Giving Per Cent. of Unlike Signs (U) and Corresponding Pearson Coefficients ($r = \cos \pi U$) of Correlations between Errors of Extent and Errors of Duration

Observer.		W.	H.	Bt.	L.
Reproducing extent	per cent. U	43	31	21	32
	r	.22	.56	.79	.54
Reproducing time	per cent. U	40	32	26	
	r	.31	.54	.67	

We may discuss the two groups separately:

(*a*) Correlations between deliberate extents and incidental durations. If in these observations the judgment of extent is based upon the perception of duration, we should expect high positive correlation—that is, the direction of the extent errors should correspond

closely to the direction of the differences in time. In fact, if the speed of all the movements was equal and uniform we should expect perfect correlation, and a reproduction occupying more time than its standard would also cover more space. The only factor tending to reduce the positive correlation would be variation in speed. If the standard and reproduction were made at different rates and the perception of duration were more fundamental, the reproduction, if slower, would be shorter, if faster it would be longer, than the standard, and the errors for extent would be greater than for (incidental) time. We have found just the reverse to be true (Table XIX.). Consequently, if the perception of duration is here serving as the basis of judgment, we must suppose that the speed is practically equal and uniform in the case of standard and reproduction. In which case very high positive correlation is required. A chance relation will be indicated by $r = 0$ in which case per cent. $U = 50$. The actually obtained correlations are, as a matter of fact, positive and rather high in three cases, in the other case positive though rather low, the four r's being .22, .56, .79 and .54, averaging .53. As far as these figures go there seems to be a pretty strong tendency for errors in extent to correspond to errors in duration.

(*b*) Correlations between deliberate durations and incidental extents. There are two conceivable ways of approaching these data:

(1) Let us suppose the perception of time to be fundamental and the perception of extent derived. Then in the reproductions of durations we have no right to expect close correspondence of the incidental extents except in so far as there is, on the part of the observer, an habitual tendency to make movements agree in both respects. At any rate, we should expect the extent errors to be greater than those for duration. But we actually do find almost as high correlation in this case as in the preceding section, the three r's being .31, .54 and .67, averaging .51, as against .53 in the reproductions of extent. Moreover, if we compare the accuracy of the deliberately reproduced durations with the incidental variations of the extents (Table XIX.) we find the V.E.'s to be indistinguishable and the C.E.'s in two cases (H. and Bt.) actually smaller for extent, while for W. the C.E. for extent is slightly larger than for time, though the probable errors of the two do not allow them to be at all sharply distinguished.

(2) Let us suppose, for sake of comparison, that the perception of duration is based on that of extent. We should thus expect, on the same argument as that outlined in section (*a*), high positive correlation between extent and time. And, as already pointed out, we get a coefficient (.51) practically as high as in section (*a*), (.53). Following out the argument of section (*a*), we find indications of a dependence of the time judgment on the perception of extent.

The method of correlation, then, affords no very conclusive evidence on the question of primitiveness. All the coefficients show is that there is a considerable positive correlation between extent and duration, no matter which factor the observer is deliberately trying to reproduce.

The result of the observers' guesses as to the probable direction of their errors have yet to be presented. The per cent. of right guesses for all the different cases is shown in Table XXII., which shows the correspondence of the guesses for both factors in each experiment.

TABLE XXII

Task.	Factor.	W.	H.	Bt.	L.
		Per Cent. Right.	Per Cent. Right.	Per Cent. Right.	Per Cent. Right.
Times to be equal.	Times.	46	52	61	
	Extents.	49	56	65	
Extents to be equal.	Extents.	59	54	64	60
	Times.	53	58	63	56

The observer guessed with respect to the deliberate magnitude only, but the table gives the correspondence of these + or — guesses with the + and — variations of both the deliberate and the incidental magnitudes.

We have found (Table XIX.) that in reproductions of extent, extent falls out more accurately than time. Table XXIII. shows that in three cases out of four the guesses agreed more closely with the actual relations of the extents than with those of the durations. There is here a suggestion that the subsequent guess, and, supposedly, the initial judgment, were made on the basis of the factor explicitly attended to (extent). However, the same relation holds in the experiments on reproduction of time. Even here the guesses correspond more closely with the actual relations of the extents, although we saw (Table XX.) that there is no clear difference in accuracy of reproduction. The percentages of right cases are not high, as 50 per cent. would indicate only chance relationship. Moreover, the reliability of a statement of per cent. of right guesses lying within quite a wide region above and below 50 per cent. is not great. The reliability of any one of the figures in Table XXII. may be calculated from the formula $\sigma = \sqrt{pq/n}$, in which p = proportion of cases in which the event occurs (per cent. of right guesses), and q = proportion of cases in which the event does not occur (per cent. of wrong guesses). In all these cases n, the total number of guesses, is about 400. This gives a probable error of about 1.7 per cent. for

each of these figures, which gives a low reliability to the differences actually shown. But the fact that in six out of the seven cases the guesses correspond more closely to the actual errors of the extents than to those of the times is unfavorable to the hypothesis that it is the perception of time on which the judgment of extent is based. Another argument in the same direction is the fact that the proportion of right guesses in experiments on the reproduction of extent is greater, for all observers, than the proportion of right guesses in the experiments on reproduction of duration.

These considerations seem to have additional significance when taken in connection with the comparison of the accuracy of reproduction for deliberate and incidental extents. Extents are seen to agree as closely when the observers are reproducing time as when they are attending to the extents, though it is not true that times incidentally measured are as accurately reproduced as those deliberately made. Instead of finding judgments of extent dependent on the perception of time we find indications of the more primitive character of the judgment of extent. At least the same argument which in experiments 1 and 2 excluded time as a factor of the judgment of extent now leads us to conceive the possible importance of the perception of extent in the process of reproducing duration. The judgment, it is true, may not be expressed in spatial terms—it may be, on the other hand, that the fundamental perception is of speed or rate of movement, and that the agreement of the extents is merely incidental to the reproduction of the speed. Reproduction of speed would call for nearly equal force or energy of contraction, and result in the production of a movement tending to agree in all respects—speed, extent and duration. That movements do tend to agree in all their attributes and that observers tend not so much to reproduce particular characteristics as to repeat the previous performance in its entirety we have already seen in Chapter II. And in the measurements of speed in in that chapter, for just such movements as those used in the present experiment, we found deviations of only 2 per cent. average in movements at the rate of 100 mm. per second. No such per cent. accuracy is found for either extent or time. Comparison of the agreement of the incidentally reproduced extents with that of the deliberately reproduced durations leads to pretty much the same situation. The V.E.'s are nearly equal, the A.E.'s deviate in different directions for the different observers, and then only by 4 or 5 per cent.

The idea of a direct sense for velocity is not a new one, having been suggested by Woodworth and asserted by Jaensch. That the perception of extent is influenced by the speed of the movement was

long ago demonstrated by Goldscheider, who found the limen to decrease as the rapidity of movement increased. After the sensations of strain and resistance, the sensations attending different velocities seem to afford the closest approximation, in the perception of movement, to a graduated intensive series, and the actual existence of such a series is easily observed introspectively. Intensive differences in the sensations localized chiefly in the elbow joint can be distinctly felt in making the same forearm movement at different velocities. The fact that the limen decreases with higher rates of movement indicates the intensifying effect of speed on a subliminal stimulus, and if, as introspection shows, changes in velocity exert this intensifying influence on supraliminal sensations as well, we have provided a thoroughly adequate basis for direct perceptions of speed, without reference to the elements of either extent or duration. Although, physically, the calculation of velocity involves a relation of distance and duration ($V=S/T$), the judgment in consciousness may not be made in terms of such a formula. The assertion of a "special sense" for speed must not be misunderstood nor carried too far. It is a "special sense" in the same way that the perception of the position of our limbs is mediated by a "special sense." The statement means merely that among the manifold qualities, intensities and interrelations of sensations afforded by movements of the limbs, certain have become, through long experience, associated so intimately with speed differences that the passage from quality, intensity and interrelation of sensation to judgment of speed no longer follows a round-about path of inference which may or may not once have been necessary, but is direct and immediate—an empirical system of speed signs has been evolved. Given a movement in process of execution and the possibility of knowing the position of my limb at a given point, I am able to tell, apparently from the intensity of certain movement sensations at the point in question, the approximate or relative speed of my movement, without reference to the actual or subsequent extent of the movement or to the time elapsed since its initiation. Indeed the speed up to the time of judgment may have been irregular. The present speed may have begun only a moment before my judgment is made, and may bear no relation at all to the total extent or duration of the movement. The anatomical source of the sensations on the intensity, quality or interrelations of which the judgment is based need not concern us in this connection.

CHAPTER V

Memory for Extent and Duration

While performing the experiments on the relation between the extent and duration of movements occasion was taken to observe the effect on accuracy of the interval elapsing between the standard movement and the attempt to reproduce it. Among the many researches on sense memory that have been reported several include experiments on extent and direction of movement and on the movement sensations involved in lifting weights. Less attention has been given to the question of memory for time intervals. A brief review of the chief studies in which memory for magnitudes in the perception of which the sense of movement is involved will serve to introduce the results of the present experiments on this point.

Cattell and Fullerton,[1] in studying memory for lifted weights, employed the method of right and wrong cases on ten observers, using a normal weight of about 100 grams and a difference of 8 grams. Seven intervals were used, ranging from 1 to 61 seconds. The approximate probable error did not seem to increase so long as the interval did not exceed 9 seconds, but beyond this point increased by about one third, but again remained rather constant for the intervals of 15, 31 and 61 seconds. "The memory image seems to last up to 9 seconds, after which the observer does not so much compare the sensations as decide on the approximate intensity of each sensation separately and compare the decisions." Lewy[2] investigated memory for the length of visual lines ranging from 20 to 200 mm., using 9 intervals between 1 and 60 seconds. The error was found to increase with the length of the interval, slowly up to 10 seconds and then more rapidly, though it was greater at 1 second than at 2. Th. Schneider[3] worked with curvilinear movements of the hand, rotating on the wrist joint. The standard was given by the method of "impact" and 6,000 experiments were made on three subjects by the method of average error of reproduction. The magnitudes lay between 70 and 100 mm. and the intervals varied from ½ to 15 minutes. Up to 2 minutes the error remained quite constant (about 3 per cent.), then increased slowly to 5½ per cent. after an interval of 15 minutes. Delabarre[4] made a few preliminary tests on the influence of time

[1] "Small Differences," p. 147.

[2] *Zeit. f. Psychol. u. Physiol. d. Sinn.*, 8, 230, 1895.

[3] "La Memoire des Mouvements Actifs," Diss., Juriew, 1894.

[4] "Bewegungsempfindungen," 105.

interval. He found 4 seconds to be the most favorable period, though wide deviations ranging to 29 seconds are said to have made no apparent difference. Jastrow[5] found memory for both visual and tactual extents to be extremely accurate and to be almost as faultless after a lapse of several days as after a few minutes. Münsterberg[6] reports experiments by Slatopolski on memory for vertical movements (5 to 50 cm.) of the arm. Intervals of 1 to 60 seconds were employed on four subjects, the standard being given by the method of "impact." The average error of reproduction, which varied from 10 per cent. to 100 per cent. with the magnitude of the standard, was found to decrease until the interval of 10 seconds was reached, after which it increased again, being about the same for 1 minute as for 1 second.

Landau[7] performed a great many experiments on memory for the extent of active and passive movements, using three observers, and testing accuracy of recognition after intervals ranging from 10 seconds to 6 minutes. He claims to have found the error to be quite uniform for intervals of less than 1 minute, but to increase considerably after 3 or 4 minutes. His tables, however, show a pretty regular decrease in the percentage of right cases, from about 73 per cent. at 10 seconds to a chance relationship at 5 minutes. Weber,[8] Courtier,[9] Vaschide[10] and Beaunis[11] have also reported more or less complete experiments on memory for extent.

On the question of memory for time intervals the early experiments of Paneth[12] are the only ones I have been able to find recorded. He found that the "sharpness of the memory image" of such intervals decreases so little in 5 minutes that no change could be detected. Larger intervals were not tried. Kennedy, reviewing the experimental work on memory up to 1898, concludes that "while memory for words, pitch, space, etc., falls off rapidly in respect to accuracy as the time interval increases, memory for time itself, so far as has been investigated, shows almost no diminution of accuracy as the time interval increases."[13]

Extent.—In the present series of experiments memory for extent was studied in the case of four observers. With three of these the

[5] *Mind*, **11**, 552, 1902.

[6] *Beitrage*, **4**, 69–88.

[7] *Wissench. Rev.*, 1896.

[8] Wagner's "Handwörterbuch der Physiol.," 3, p. 2.

[9] "Drit. Int. Cong. f. Psychol.," 1896, 238.

[10] *Ibid.*, 454.

[11] *Rev. Philos.*, **25**, 369.

[12] *Centralbl. f. Physiol.*, **4**, 81–83, 1890.

[13] *Psychol. Rev.*, **5**, 483, 1898.

standard magnitudes ranged from 100 mm. to 400 mm., and the continuous method was used, the terminal point of the first movement serving as the starting point for the second. With the fourth observer the magnitudes ranged from 150 mm. to 600 mm. and the "successive" method was used, the arm being returned to its initial position after passing over the standard distance, the two movements being thus made over the same stretch of track. Five different intervals were used, viz., 2, 5, 10, 15 and 30 seconds, the interval being in each case the time between the termination of the standard and the beginning of the reproduction. The signal from the sound hammer served to determine the magnitude of the standard. The intervals were measured by the swings of a seconds-pendulum, the "Now" of the operator being the signal for the beginning of the second movement. After the reproduction the observer guessed as to the probable direction of his error by judging whether it was "greater" or "less." The car was then returned to the starting point and the next trial made. In order to avoid fatigue in the extended arm in the case of the long intervals a horizontal desk-like shelf was placed along the track on the observer's side, at a height which allowed the hand and wrist to be supported while the car remained in position. The finger could thus be raised from the car and the feeling of cramp relieved.

In the calculation of error the magnitudes have been arranged in 5 or 6 groups—100 mm. to 150 mm., 150 mm. to 200 mm., etc. In the case of three subjects 15 magnitudes, falling between the upper and lower limits of each group, were given for each interval, making 75 trials of each interval for observer L. and 90 for each interval with W. and H., making totals of 375 trials for L. and 450 each for W. and H. In the case of Bt. 50 trials for each interval were given, making 250 trials. The per cent. error for each trial was calculated and the individual errors averaged to get the group average. This final error was then analyzed into constant and variable errors. The grand average for the total range, for each interval, was then computed and is indicated in the following curves.

The particular errors for each observer may be found in the proper table in Chapter IV.

The results from all four subjects are quite uniform. A statement of the general tendency will depend chiefly on which one of the three measures of error is chosen. In all three cases the gross average error of reproduction increases in general, with the length of the interval. All three show an increase of gross error after 2 sec., which either falls slightly or remains constant at about 15 sec. After this point the curves for the A.E. no longer agree, those for

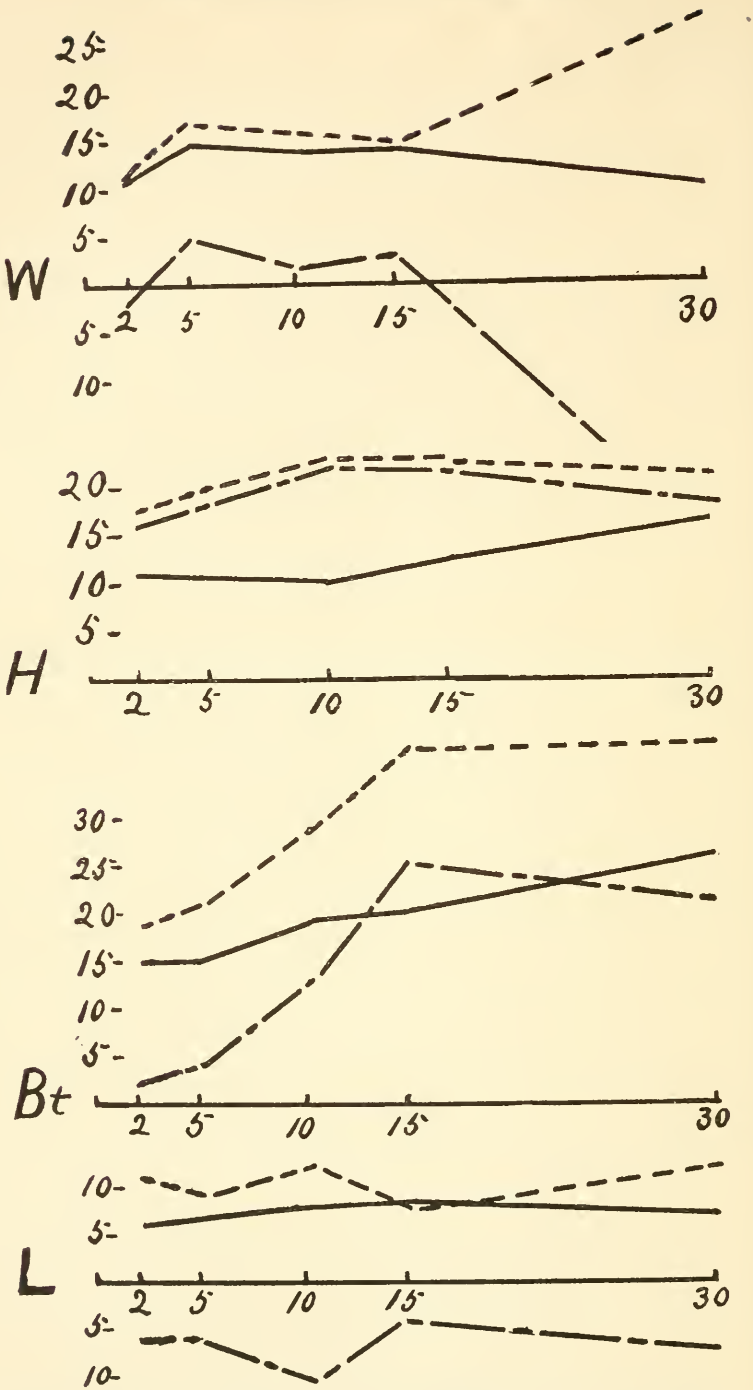

FIG. 2. Memory for Extent. Curves showing increase in error with increasing interval between standard and reproduction.

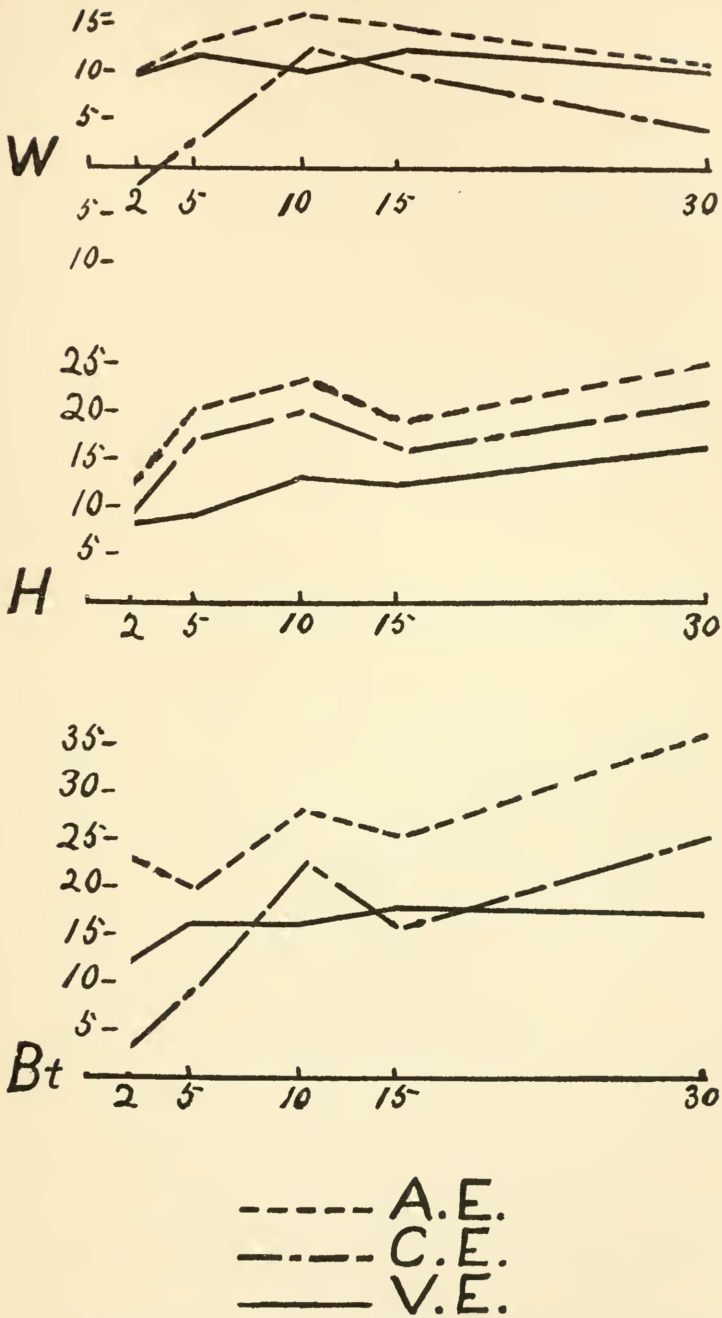

FIG. 3. Memory for Duration. Curves showing increase in error with increasing interval between standard and reproduction.

H. and Bt. remaining on about the same level, that for L. rising to its previous maximum, while that for W. rises considerably. But these effects all appear to be due to the constant error, the curves for which are seen to have the same general direction. The C.E. becomes more positive up to 10 or 15 sec., and then falls again, becoming either a smaller positive error or, as in the case of W., considerably negative. The variable error, however, seems to be only slightly, if at all, affected by the increase in time interval. For L. the level is practically uniform, for H. the only considerable increase is at 30 sec., while for W. the errors at 2 sec. and at 30 sec. are slightly lower than the level of the other three points; Bt.'s V.E., however, increases regularly, from 15 per cent. at 2 sec. to 26 per cent. at 30 sec. Since these tests were extended over a considerable period of time the changes in the constant error may easily enough be due to factors other than the variation of the time interval. Usually the trials made in a given sitting were for one or at most three intervals, the memory problem being rather incidental to the experiment proper. Under the circumstances the variable error is the most reliable measure of accuracy. From its relative constancy we may conclude that, within the limits of the investigation, the accuracy of reproduction, as measured by the variable error, is not influenced by changes in the time interval. This conclusion seems to be further confirmed by the fact that the proportion of right to wrong guesses does not decrease as the time interval lengthens. Such slight change as does occur is on the whole in the reverse direction, the proportion of right to wrong guesses increasing slightly for the longer intervals (see Table XXIII.). In the case of Bt., indeed, this increase is rather striking.

Time.—The procedure in the experiments on memory for duration was the same as in those on extent. The standard magnitudes ranged from 1 sec. to $3\frac{1}{2}$ sec., and have been classified under five groups. The calculation here was the same as in the case of extent. The subjects were W., H. and Bt., and 75 trials were made for each interval between the standard and the reproduction. The continuous method was used, the standard duration being determined as before, by the checking of the movement at the signal from the sound hammer. At the word of the operator, after the appropriate interval, the observer went on to reproduce the duration of the standard movement, and guessed as to the probable direction of his error. The results are shown in curves 6, 7 and 8, and in the appropriate tables in Chapter IV. Here again the three observers agree pretty closely. A.E.'s and C.E.'s increase up to 10 sec. Of course the determining factor here is the C.E. for changes in the

A.E. merely reflect changes in the C.E. In all three cases the C.E. drops at 15 sec., rising again with H. and Bt., to the maximum at 30 sec., but dropping still further in the case of W. The V.E. undergoes little change. That for W. remains on practically the same level throughout, agreeing with the results of Paneth's experiments. With Bt. and H. there is, however, a rather uniform, though slight, decrease in accuracy, the V.E.'s increasing from 12 per cent. to 17 per cent. with Bt. and from 8 per cent. to 16 per cent. with H.

TABLE XXIII

Proportion of Right to Wrong Guesses with Increasing Interval

		2 sec.	5 sec.	10 sec.	15 sec.	30 sec.
Extent.	L.	1.2	0.9	1.8	1.5	1.9
	W.	1.1	1.8	1.7	2.2	0.8
	H.	1.0	1.2	1.0	1.3	1.5
	Bt.	2.0	1.4	2.4	2.7	3.3
Time.	W.	1.0	0.7	0.6	0.8	1.1
	H.	0.8	0.7	1.6	1.6	1.1
	Bt.	0.7	3.0	1.7	1.8	1.5

In these two cases the loss of accuracy accords, on the whole, with the "law of forgetting," as it is usually stated, the error increasing rapidly at first and then more slowly. Here, as in the case of extent, the proportion of right to wrong guesses fails to indicate any decrease in accuracy of memory (see Table XXIII.). For W. the proportions remain quite constant, for H. there is a considerable increase in the proportion of right guesses as the interval is lengthened, while for Bt. the minimum is at 2 sec., the maximum at 5 sec., the proportion for the greater intervals remaining equal.

CHAPTER VI

Influence of the Degree of Contraction

One of the points on which various writers have differed is a phenomenon first noted by Loeb.[1] He found that short movements executed in different portions of the possible range of rotation of a joint are not estimated with equal accuracy. In his experiments the movement made under conditions of less contraction of the acting muscle was underestimated as compared with one made under a greater degree of contraction. This fact was used by Loeb in support of the theory of innervation. He held that the objective shortness of the second movement was to be explained by supposing that, by virtue of the partial contraction already involved in making the first movement, further contraction was more difficult. As a consequence the same innervation produced a smaller change, but since equal innervations were made or intended, the two movements appeared of equal length.

Külpe denies the validity of Loeb's figures and ascribes the supposed phenomenon to "erroneous evaluation of experimental results."[2] Delabarre[3] suggests that the results are probably reliable and explains them on the basis of the supposed greater difficulty of the movement under greater degree of contraction. But this feeling of greater difficulty does not, for Delabarre, involve innervation feelings. However, he finds the illusion to occur only under considerable differences of contraction. Angier[4] finds it not to occur under any circumstances. Kramer and Moskiewicz[5] find the illusion always to occur, and in varying positions and directions of movement. They explain it by the principle of unfamiliarity, the theory being that in the unfamiliar position the hand makes slower movements which are thus made shorter in order to be equal to longer movements made more quickly. Since the sensation complexes are different in the two cases the comparison must be made on the basis of some common quality. The only common quality present is the duration. Consequently these investigators surmise that the durations of the two movements are equal when the extents appear to be,

[1] *Pflügers Archives*, **46**, 1–46, 1890.
[2] "Outlines of Psychology," p. 342.
[3] "Bewegungsempfindungen," p. 90.
[4] *Zeit. f. Psychol.*, **39**, 430, 1905.
[5] *Ibid.*, **25**, 101–125, 1901.

but they made no attempt to support their theory by actual measurement of the time.

Woodworth[6] takes exception to the form of the preceding generalizations. His results show that of the introspectively equal segments of the total excursion, the objectively greater extents do not occur at the beginning but in the middle, while the movements at both extremes are overestimated, *i. e.*,—are shorter in execution. Woodworth accepts Delabarre's suggestion of the relative ease of performance. Myers[7] accepts Woodworth's form of the illusion and Delabarre's explanation.

The following experiments were performed in order to confirm either Loeb's or Woodworth's results or to show that no constant errors are to be found, and to test the "equal duration" hypothesis

TABLE XXIV

Extent of Successive Movements Intended to be Equal

		mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.
I.	R.	321	*8*					257	*9*					223	*7*
	W.	260	*12*					279	*13*					240	*21*
	B.	232	*9*					272	*6*					232	*7*
II.	R.	203	*13*			201	*7*			175	*6*			166	*4*
	W.	207	*15*			246	*10*			228	*10*			196	*10*
III.	R.	162	*13*	156	*12*	134	*10*	123	*8*	110	*10*	115	*11*	99	*6*
	W.	139	*16*	158	*13*	166	*13*	149	*12*	131	*11*	126	*10*		
	B.	105	*14*	122	*9*	137	*15*	129	*2*	116	*9*	95	*7*	89	*7*
	Bt.	156	*15*	177	*19*	143	*11*	132	*9*	108	*9*	103	*8*		
	V.	158	*8*	171	*10*	161	*2*	139	*8*	126	*11*	116	*10*		
	C.	136	*10*	140	*13*	140	*10*	121	*8*	111	*9*	124	*8*		

TABLE XXV

Duration of Movements whose Extent is Shown in Table XXIV

		sec.	% A.D.	sec.	% A.D.	sec.	% A.D	sec.	% A.D.	sec.	% A.D.	sec.	% A.D.	sec.	% A.D.
I.	R.	3.13	*9*					2.97	*11*					3.14	*10*
	W.	1.38	*38*					1.32	*35*					1.39	*35*
	B.	1.12	*13*					.96	*10*					.92	*9*
II.	R.	2.28	*19*			2.13	*9*			2.08	*7*			2.24	*15*
	W.	1.34	*37*			1.14	*33*			1.16	*41*			1.35	*42*
III.	R.	2.16	*24*	2.09	*20*	2.07	*21*	2.08	*21*	1.93	*18*	2.17	*18*	1.97	*27*
	W.	1.10	*35*	1.20	*35*	1.15	*33*	1.04	*55*	1.00	*32*	1.08	*35*		
	B.	1.12	*14*	1.03	*15*	1.00	*15*	.97	*9*	.91	*12*	.78	*10*	.86	*9*
	Bt.	.89	*8*	.95	*12*	.88	*14*	.87	*13*	.83	*11*	.85	*12*		
	V.	.80	*15*	.76	*17*	.70	*11*	.68	*10*	.68	*15*	.67	*9*		
	C.	.80	*11*	.75	*12*	.76	*13*	.70	*13*	.71	*11*	.75	*11*		

[6] "The Accuracy of Voluntary Movement," p. 79.

[7] "Experimental Psychology," p. 73. Longmans, 1909.

of Kramer and Moskiewicz. The experiment consists of three series, in all of which the extents were recorded in millimeters and the durations in hundredths of a second.

In Series I. the total excursion was divided into three movements of introspectively equal extents, in Series II. into four such movements. In Series III., after a few preliminary trials in order to get the total number of movements into the single excursion, the subject made six (in two cases seven) successive movements, here again of apparently equal length. In cases R., W. and B. the length of the interval between movements was left entirely to the preference of the subject. In cases Bt., V. and C., each movement was made at a signal by the operator. In this way it was possible to vary the interval between movements and thus avoid any tendency to mere rhythmical performance on the part of the subject. Series I. and II. show the average results of 10 trials and Series III. of 20 trials for each subject, a total of 930 movements. Tables XVIV. and XXV. give the average movement in each position, in millimeters, and its average deviation in per cent. for both extent and duration. Table XXVI. gives the average of the first movements and the positive or negative deviation (A.E.) in per cent. of each of the successive movements from this average, both for extent and duration as well as the variability (V.E.) of all the movements from their average.

Extent.—In Series I., for both W. and B., the middle segment is longer than either the first or third. In Series II., for W., both the second and third segments are longer than either the first or fourth. In Series III., except for subject R., and one additional instance, the first segment is shorter than either the second or third and in two cases than the fourth segment, while beyond the approximate middle of the excursion the segments decrease again, still more rapidly than they increased at the beginning. Thus in all but one subject the results correspond with those obtained by Woodworth. The divergence in the case of R. is probably due to the fact that of the six subjects he was by far the tallest and had the longest arm. The range employed did not constitute his maximum excursion, and from the position assumed before the apparatus the first part of the total swing was the part not used. The degree of contraction of any one muscle or single set of muscles does not afford adequate basis for generalization, even disregarding the fact that somewhat different sets are likely to be employed in the execution of different segments. It should also be noted that these results are just the reverse of what one should expect if the judgment of extent were based on the angle of rotation at the joint. For the same angle, reproduced at

TABLE XXVI

	Extent.										Time.							
	Observer.	Standard. Unit, 1 mm.	Per Cent. Error.						A.E. Per Cent.	V.E. Per Cent.	Standard. Unit, 1 sec.	Per Cent. Error.						A.E. Per Cent.	V.E. Per Cent.
I.	R.	321			—20			—31	26	13	3.13			— 5			+ 0.3	3	2
	W.	260			+ 7			— 7	7	5	1.38			— 4			+ 0.7	2.5	2
	B.	232			+17			0	9	8	1.12			— 5			—18	12	8
II.	R.	203		—10		—14		—18	14	9	2.28		— 7		— 9		— 2	6	4
	W.	227		+ 8		+ 0.5		—14	7	6	1.34		—15		—13		+ 0.7	9	8
III.	R.	162	— 4	—17	—24	—32	—29	—39	24	15	2.16	—3	— 4	— 4	—10	+ 0.5	— 9	5	3
	W.	139	+13	+19	+ 7	— 6	— 9		11	9	1.10	+9	+ 5	— 5	— 9	— 2		6	5
	B.	105	+16	+30	+23	+10	—10	—15	16	13	1.12	—8	—11	—13	—19	—30	—23	15	10
	Bt.	156	+12	— 8	—15	—31	—34		20	16	.89	+7	— 1	— 2	— 7	— 4		3.5	3
	V.	158	+ 8	+ 2	—12	—22	—27		14	13	.80	—5	—12	—15	—15	—16		12	6
	C.	136	+ 3	+ 3	—11	—18	— 9		9	8	.80	—6	— 5	—13	—11	— 6		8	3
Averages									14	11	Averages							7	5

the shoulder joint, would mean a greater movement at the extremes of this total rectilinear excursion than in the middle, and in the attempt to reproduce extent the middle segments would be shorter, those at the extremes longer.

Duration.—In the case of the times the segments never increase throughout the total excursion. On the other hand, in 7 cases the extreme segments are greater than the intermediate. This indicates a tendency for the extreme segments, both initial and terminal, to be greater and the intermediate less than the average of the group. This is especially clear in Series I. and II., where the differences in degree of contraction for the separate movements are greater. Correlating ($r = \cos \pi U$) the extents with the durations of the respective segments, we get the following coefficients:

Observer.		R.	W.	B.	Bt.	V.	C.
Series I.	$r=$	+.51	—.51	—.51			
II.	$r=$	0	—.71				
III.	$r=$	+.43	+.71	+.61	+.97	+.86	+.86

When the differences between the positions of the successive segments are considerable, as in Series I. and II., the correlation is negative—the longer movements occupy the shorter times, but when the differences in position are less, as in Series III., the correlation is positive—extents and durations tend, on the whole, to vary in the same direction.

Inspection of the average deviations of the individual movements from their averages, as shown in Tables XXIV. and XXV., shows that the extents included in any given segment average are much more constant and uniform than the durations. That is, their average deviations from the average of their respective groups are smaller, the grand average for extent being only 10 per cent. as against 19 per cent. for duration. This difference, however, is not particularly significant, since no special effort was made to keep equal the magnitudes ranging around a given segment. Moreover, there was more chance for variation in the time than in the extent, since the observer's attention was never explicitly called to the duration of his movements and there was no attempt to keep the speed constant. The attempt was always, having made a first movement, to make all succeeding movements of the excursion in question equal in extent to this first, without reference to the magnitude of corresponding segments of other excursions.

Table XXVI. shows the relative accuracy of this performance with respect to the deliberate extents and the incidental times as well. In this table the A.E. represents the average deviation from

the standard, while the V.E. represents the per cent. variation from the average of all the segments of the group. The final A.E. and V.E. for extent (14 per cent. and 11 per cent.) are twice as large as the corresponding errors for duration (7 per cent. and 5 per cent.). The durations are more nearly equal than the extents, just as was the case in Chapter IV. As the figures stand, they tend pretty strongly to confirm the conjecture of Kramer and Moskiewicz.

But this closer agreement does not in itself suffice to demonstrate the perception of time to be any more fundamental than the perception of extent. As in most cases of naturally reproduced movements, there is a tendency to repeat the whole original performance (see Chapters II. and IV.), reproducing both duration and extent. In this case factors enter which disturb the spatial judgment but do not affect the temporal. The times are more nearly equal, not because they constitute a common factor which serves as a basis of comparison in reproducing extent, but simply because the change in position of the moving member introduces no factors which affect the judgment of duration. There is indeed no reason for supposing the perception of time to be based on processes in the moving member. It is probably based instead on processes of a more permanent and regular sort, taking place in other parts of the organism. But the judgment of extent is subject to every local change in position, strain, ease of movement, familiarity, inertia, etc.

In producing the illusion of extent all the factors mentioned by the earlier writers are probably effective, allowing for the modification of Loeb's "innervation" theory made by contemporary psychology. These factors seem to fall into two groups: (1) conditions of performance and (2) conditions of perception.

1. *Conditions of Performance.*—It may be supposed that the first movements of the series are more difficult than later ones, since the inertia of the musculature has to be overcome in the one case but is already removed in the other. In the middle portion of the excursion movements are more easily made, since the muscle is already warmed up and in action. While at the terminal end of the excursion it may be supposed that movement is more difficult than in the middle portion because of the greater degree of contraction, entailing greater innervation, or because of the unfamiliarity of the movement. This last factor may affect both ends of the series. Movements here, being less familiar, and the signs which indicate extent being less thoroughly systematized and learned, tend to be made with greater caution. That they are made more slowly is clear from Table XXVI. When the duration of the standard movement has elapsed there is a strong disposition to feel the movement as completed, since its tem-

poral factor has been approximately reproduced. The movement is thus stopped somewhat short of the proper extent, a kind of compromise being effected in which both spatial and temporal accuracy are partially sacrificed, the durations tending on the whole to fall out slightly too long and the extents too short.

2. *Conditions of Perception.*—Delabarre's suggestion that anything which increases the sensory elements of a movement increases its apparent magnitude, though untenable as a generalization, may be applied here with advantage. There is no doubt, introspectively, that at either extreme of the arm's excursion the sensations resulting from a given objective change in position of the limb are relatively intensified. The member is approaching its limit of movement, the tension of muscles and tendons is approaching a maximum as the degree of contraction increases, the skin over the joint is stretched, the subcutaneous tissues are more firmly compressed about the fulcrum of the joint. The sensory elements of any movement made under these conditions will be relatively increased and in so far as extent of movement is judged in terms of intensity of sensation, we should have the Loeb illusion at both extremes of the total swing without considering the difficulty of performance, either as a result of inertia, degree of contraction or unfamiliarity. Moreover, we would expect more complete and prompt adaptation to the sensations aroused by the more familiar movements in the central portion of the excursion and this again would produce a relative decrease in the sensory elements of such movements.

CHAPTER VII

Criteria of the Judgment of Extent

The greater part of the work on movement has been topographical in motive and in method, consisting of observations of motor ability and accuracy under definite experimental or pathological conditions or of attempts to localize anatomically the source of the sensations on which specific judgments are based. Interesting as these results may be to the physiologist or physician, they throw little light on the processes of discrimination, recognition and comparison involved in our judgments concerning movements. In fact there seems to be a kind of "anatomist's fallacy" in such a procedure, at least so far as the psychology of movement is concerned, for it seems to proceed on the tacit assumption that the sensation is, psychologically, what it is anatomically. And, as we might expect, the topographical procedure has led into all kinds of disagreement. Nowhere is this disagreement more apparent than in the matter of the criteria or differentiæ of the judgment of extent. The chief cause of disagreement here seems to have been the desire to simplify the "muscle sense," to trace, if possible, the sensation of movement to a single anatomical source. This term "muscle sense" has been used to designate the whole group of articular, tendinous, muscular, cutaneous and visual elements that go to make up the kinesthetic perception. That the mere sensation of movement may be mediated by any or all of these has been pretty generally agreed, but when specific topics are concerned—the judgments of extent, force, time and direction of movement—opinion is not nearly so unanimous.

After a great number of experiments of the topographical sort, Goldscheider[1] concluded that the joint sensations afford the chief criteria for the judgment of extent and direction of movement. Attempts were made to get a pure muscle sensation isolated from other elements of the kinesthetic sensation. The skin over a muscle was anesthetized and the muscle stimulated electrically. The diffuse sensation produced was said not in the least to resemble the sensation of movement. When the joint alone was anesthetized the consciousness of movement became so blunt that it was evident that the feeling of contraction could not be used for fine discrimination of extents, while anesthesia of the skin produced no disturbance of space perception. On the basis of these and similar experiments

[1] A. Goldscheider, "Untersuchungen über den Muskelsinn," 369 ff.

Goldscheider concludes that "muscle sensations, which were formerly accorded the leading rôle in the cognition of weight and in the estimation of the magnitude and direction of movement, do not appear at all except as a result of intensive stimulation, great fatigue or in the form of muscular pain."

Külpe[2] accepts Goldscheider's conclusion, with certain amplifications of his own. "It may be conjectured *a priori* that muscular and tendinous sensations can not form the ground of our judgment of the position and movement of our limbs in the absence of visual perception. There is no proportionality between the extent and duration of a movement and the possible concomitant excitations in muscle and tendon." "On the other hand, the relation between the positions of the articular surfaces as regards each other and positions or movements of the limbs is just as simple as that between the different parts of the skin or retina and the points from which they are stimulated. We see, therefore, that the articular sensibility furnishes us the real basis of our perception of the position and movements of the limbs where an appeal to vision is excluded." Kramer and Moskiewicz confirm this conclusion by saying: "Sensations arising from the processes of tension of the muscles are unessential to inform us concerning the judgment of the position or movement involved."[3] James, in turn, accepts the theory on the basis, chiefly of Goldscheider's results: "We indubitably localize the finger tip at the successive points of its path by means of the sensations which we receive from our joints."[4]

In striking contrast with this position are the more recent statements of Pillsbury and of Reichardt, leading back to the older position of Brown and Delboeuf. Reichardt,[5] working on the illusions of passive movement, claims that the sense of position is not mediated by the part moved but by processes in the moving muscle. Pillsbury[6] finds that "the sensitivity of joints is decreased by induction currents through the wrist and elbow as well as through the joints in question. This fact, together with the lack of anatomical evidence that the joints have sensory endings, makes it probable that the sensation of movement is derived mainly from the tendon and muscle, rather than, as Goldscheider thought, from the joints."

Under the circumstances, then, we should expect somebody else to abstract some other element and exalt it into the position of chief

[2] "Outlines of Psychology." London, Sonnenschein, 1901, 143.

[3] *Zeit. f. Psychol.*, **25**, 105, 1901.

[4] "Principles of Psychol.," **2**, 193.

[5] *Zeit. f. Psychol.*, **40**, 430, 1906.

[6] *Amer. Jour. of Psychol.*, **12**, 346, 1901.

criterion. And this is what occurs. Bourdon[7] finds that the least perceptible tension of the skin about the dorsal joint of the finger is about .2 mm., and that this is just the tension required to allow the least perceptible movement—1 mm.—to take place. He also insists that to suppose the joint sense to be the source of criteria for judgments of extent of movement presupposes for the articular surfaces a tactual acuity much higher than that of the skin in its most sensitive parts, and that this contradicts the general rule that sensitivity decreases as we go more deeply into the interior of the body. Consequently, he concludes that the criteria of extent of movement are in all probability to be found in the tensions of the skin above the moving joint. Nevertheless we find Pillsbury saying: "That the skin does not serve as source of the sensations which indicate movement may pass without comment."

Still other facts tell against the conception of the joint linings as a "reduced map" of the extent of movements. One is the fact that our movements do not consist of simple joint movements in one direction or of combinations of such movements. Thus in the execution of a compound arm movement of any considerable magnitude the elbow joint tends to double back, beyond a certain point in the movement, retracing its original rotation but in the reverse direction. Particularly is this true if the movement approximates the rectilinear type. As a result of this it follows that the fixed point-for-point correspondence between points on the articular surfaces and points in external space is not so fixed as might at first appear. A point on the membrane lining the shoulder joint may mean almost any point in external space, depending on the complex relation of the positions of elbow, wrist and finger joints. If our most common movements or even our earliest movements consisted of rotations at a single joint, a point for point correspondence might be established. But such is not the case. From the genetic point of view at least our spatial order is built up on a basis of primitive and practical movements which are complex in character and mechanism—such movements as brushing away a fly, pulling or pushing objects to or from the body, striking a blow, raising a lever, etc. The anatomically simple single joint movement comes to be artificial, for greater speed and accuracy are undoubtedly to be gained by the complex movement. But even with these compound movements there might, it is true, be developed a system of local signs on the articular surfaces, the combinations and interrelations of which might come to mean extent of movement. Such a proposition, however, yields the whole argument for the exclusive rôle of the joint sense and affords no reason for

[7] *L'Annee de Psychol.*, **13**, 133–143, 1907.

excluding criteria afforded by sensations from muscles, tendons, skin and subcutaneous tissue.

A striking experiment by Münsterberg[8] shows that the same extent of movement may be represented in one situation (with extended forearm) by a given angular rotation, in another (with forearm flexed) by a rotation three or four times as great. This experiment alone should suffice to demonstrate the empirical basis of the judgment of extent, and to emphasize the importance of factors other than the number of degrees of joint rotation. Still further, whatever importance one may be disposed to attribute to eye movements in the perception of visual space, the fact remains that to a certain extent, even with closed eyes or in the dark room we can know with a certain degree of correctness the position of the eyes and estimate the amount of their movement, although there are no articular membranes involved.

An even clearer illustration is to be found in cases of acquired control over the ear muscles. Diligent practise since boyhood has enabled me to perform either monaural or binaural movements with considerable facility and has developed a rather definite range of recognized extents. In this case there has been neither articular surface nor even cooperation with visual criteria. Movements of the tongue are also made with great accuracy, although we do not ordinarily have occasion to apply objective scales of measurement to them.

Attempts to find a single topographical or anatomical source have thus been futile. Goldscheider's experiment, which for James "completely established" the rôle of the joint sense is contradicted by Pillsbury's results. Adherence to Külpe's suggestion of the accurate correspondence of points on articular surfaces with points in external space requires a tactual acuity which Bourdon can not accept, and nerve endings in the joint linings, which have not yet been satisfactorily demonstrated. Reichardt's attribution of the sensations indicating extent to the processes taking place in the moving muscle is discounted by Duchenne's patients, in whom insensibility of muscles was found along with intact perception of movement. Bourdon's attempt to refer the sensations to skin tension over the moving joint is contradicted by Goldscheider's subjects with anesthetized skin but unimpaired perception of movement. And these topographical attempts fail because, as it appears, sensations do come from many sources and any sensation which can aid in the differentiation of one movement from another serves to identify that movement when it occurs again.

More conciliatory is the statement of Delabarre to the effect that

[8] *Beiträge*, 1892, IV., 178–191.

"movements are judged equal when their sensory elements are equal," although the precise nature of such an equality is not apparent. Aside from the possible tautology of the statement, it is not clear how such heterogeneous elements as duration, speed, force, strain, position, are commensurable. The equality can hardly be of an intensive character, for two excursions may be equal in extent and yet afford sensations of strain that are exceedingly disproportionate to the error in apparent magnitude. A better statement would probably be the one we have already suggested, viz., that movements are judged to be equal which have been *learned to be equal*—that judgment and discrimination are not based on anatomy, nor even on an intensive psychophysical relation between magnitude of stimulus and intensity or extensity of sensation, but are inferential processes, founded in the empirically coordinated consequences of experience. Innumerable secondary and essentially unrelated criteria may be utilized in the recognition and in the judgment, which is a purely qualitative one, not "How much joint movement or skin tension is now felt?" but "What signs can I find to help me recognize this movement among the many other movements with which I am somewhat familiar?" Titchener[9] finds that so irrelevant a thing anatomically as the way in which the arm fell down against the side after completing the movement was in one case the basis for the judgment of extent of arm movement. Even in judgments of resistance, as Bolton[10] has pointed out: "Perceptions of greater do not necessarily rest upon greater perceptions and a sensation of intensity is not an intense sensation." "Judgments of same and heavier are inferences from certain facts, and these facts are the excitations of areas in the one case that remain unaffected in the other."

Woodworth concludes that "there must be a sense of the extent of movement, a sense which is not reducible to a sense either of its force or of its duration or of its initial and terminal positions."[11] There is no contradiction between such a statement and the one just made. To say that we have a direct and immediate sense of the extent of movement may mean just what is here suggested—that a variety of qualitative signs have been learned to mean movements of definite magnitudes, irrespective of the extensity attribute of the particular muscular, tendinous, articular or cutaneous sensations involved. Instead of insisting on the prominence of any one of

[9] "Exper. Psychol.," Vol. 2, pt. 2, 260.

[10] Bolton and Withey, "On the Relation of Muscle Sense to Pressure Sense," *Univ. of Nebraska Studies*, 1907, 7, 21.

[11] "Accuracy of Voluntary Movement," *Psychol. Rev.*, Mon. Supp., 13, 80, 1899.

these sources it seems more satisfactory to say with Sherrington that the muscle sense is based on a "specific set of sensations obtained by specific sense organs in the muscles, joints and all the accessory organs of movement."[12] Any sensitive part that is in any way uniformly stimulated in the process of a given movement contributes its share to the character of the movement as a conscious fact, and any such contribution may be utilized in the recognition of the movement when it occurs again. But this recognition does not seem to be based on the quantitative relations of this "specific set of sensations," nor on any such geometrical correspondence as Külpe suggests. It is throughout a qualitative recognition. Out of the variety of stimulations that accompany excursions differing in direction, extent, resistance and speed, certain combinations have been learned to mean position, others distance, others resistance or strain and still others velocity, however disproportionate the extensities or intensities of the sensations in their own right. A greater intensity of sensation does not mean a greater resistance or pressure. It may mean a lesser objective stimulus under more sensitive conditions.

In studying the accuracy of space perception, therefore, and in analyzing any tendency to error found there, we are investigating just this association of sensation complex with objective meaning. From this fact great uncertainty arises in the application of the psychophysical methods to the study of movements. Observers tend here to refer every stimulus to some absolute scale of magnitudes and to estimate and compare, not by a genuine balancing of impression against impression, but by position claimed or assigned in this absolute or practical objective scale. Thus two movements of different extent are likely to be felt, not so much as "larger" or "smaller" impressions, but rather as impressions that are qualitatively different. Comparisons are seldom made in subjective terms. Since our movements are our means of voluntarily manipulating our environment, they come to be specialized for specific purposes and are thus characterized qualitatively by their function. A movement comes to be recognized as larger than another, not because it produces a more intense sensation, but because it has been *learned to be* a greater movement—a movement that will effect a greater change in an object with which we are dealing. The element of extensity involved in movement is not the primary quality of extensity attributed to all or most of our other sensations. A joint or tendon sensation or a sensation of cutaneous tension may possess an extensity of its own, but it is only empirically and after long experience that this extensity comes to mean definite extent of movement. Or

[12] Stirling, "Outlines of Practical Physiology," 578. London, Griffen, 1902.

an object or movement may come to be felt as greater than another by virtue of the fact that one excites a local sign that the other has not affected. And this local sign, once awakened, constitutes not a quantitative but a qualitative distinction. There is no more reason for supposing that the estimation of movement depends on a highly developed joint sense than there is for believing that it depends solely on any other.

SUMMARY OF EXPERIMENTAL RESULTS

Chapter I. Methods

Description of apparatus designed to record simultaneously and graphically the extent, force, duration and velocity of rectilinear arm movements.

Chapter II. The Illusion of Impact

Movements terminating in impact are affected in perception and reproduction with a large positive constant error, the magnitude of which depends on (*a*) the force of impact and (*b*) the point in the intended movement at which the impact occurs. The greater the force of impact and the less the amount of the intended movement already accomplished, the greater the illusion.

Practise without knowledge has no effect on the constant error. The result of practise with knowledge is not to decrease the illusion so much as to produce a deliberate shift in the scale of extent criteria, leading to a corresponding negative constant error in the judgment of free movements.

The illusion may be explained on the basis of (*a*) the original intention, (*b*) irradiation of the stimulus, (*c*) increase of the sensory elements of the movement complex through fusion of the shock of impact.

Chapter III. The "Indifference Point"

With respect to the experiments on extent of movement reported in this chapter:

1. No magnitude evinces any considerable constant error of reproduction when estimated out of relation to a group or series of which it is a member.

2. The same absolute magnitude may be under one circumstance an indifference point, under another affected with a positive constant error or again with a negative one.

3. A periodic indifference point can be found within the total series (S) by working with its special sections (A, B and C).

4. The gradual extension of the series limits is accompanied by a corresponding shift in the region of indifference.

5. The phenomenon of the indifference point, so far as it occurs in our spatial judgments and in our temporal judgments, at least so far as they are a function of extent of movement, is of central

origin, and its position depends on the range or limits of the magnitudes used in a given experiment.

6. The constant errors do not so much represent transformations in a memory image of the stimulus in question as they do the effect on a present judgment of the persistence of the mental set involved in the directions of previous judgments.

7. If the interval between the separate judgments is sufficient the first dispositions are soon dissipated and are no longer adequate to affect the succeeding performance.

8. In the presence of such grouped or serial magnitudes we tend to form our judgments around the mode or central tendency of the series. Toward this mean each judgment tends by virtue of a mental set corresponding to the upper and lower limits of the total range of magnitudes. This is the equivalent, for judgment, of Leuba's "law of sense memory."

Chapter IV. Relation between Extent and Duration

The four methods of separate accuracy test, confusion, correlation and correction fail to justify the assumption that the perception of any one characteristic of a movement is more primitive or fundamental than that of any other.

Extent and duration can be reproduced with about equal accuracy, the difference being slightly in favor of duration. But the incidental durations of movements intended to be of equal extent show a variable error which is greater than that of their corresponding extents as well as that of the same duration magnitudes when deliberately reproduced.

Constant errors seem to be bound up with the process of deliberate reproduction, the constant error for the magnitude attended to (extent or time) being greater than that of the magnitude incidentally reproduced (time or extent). Thus the constant errors for deliberate extent and for deliberate time are both greater than those for incidental extent and incidental time.

The coefficients of correlation show that, disregarding the magnitude of the errors, there is considerable positive correlation between their directions for extent and duration, no matter which factor is being attended to.

Subsequent guesses as to the probable direction of the error of attempts to reproduce either extent or duration correspond more closely to the actual relations of the extents than to those of the durations. The proportion of right guesses in reproduction of extent is greater than in reproduction of duration.

These facts point to separate processes of judgment for the two

magnitudes (extent and duration). There is at least no justification for the attribution of more fundamental character to the perception of either. The judgment of extent seems to be based on a system of signs which have been learned to mean extent directly. The same seems to be true of both duration and velocity.

Chapter V. Memory for Extent and Duration

Within the limits of the investigation the accuracy of reproduction of extents, as measured by the variable error, is not influenced by changes in the time interval. With respect to the constant error individual differences are shown.

The curve of memory for duration follows more closely the ordinary statement of the ''law of forgetting,'' in the case of the constant error, although the variable error undergoes little change up to an interval of 30 seconds.

Chapter VI. The Degree of Contraction

The Woodworth modification of the Loeb illusion is present in nearly every case of rectilinear arm movement. The middle segments of a total excursion are underestimated in comparison with the segments at either extreme.

When the differences in position between adjacent segments is considerable, the total swing thus consisting of but few segments (3–4), the durations show just the reverse phenomenon—initial and terminal segments frequently tending to require longer time than intermediate segments. When the differences in position are less (6 and 7 segments) the correlation is positive—extents and durations tend to vary in the same direction.

The average deviation for the durations is only about half as great as that for the extents, but this is not necessarily due to a more fundamental character of the perception of time.

There is a tendency in reproduction to repeat the original performance as a whole. By the conditions of the experiment the spatial judgment is confused while the perception of duration is undisturbed.

Chapter VII. Criteria of the Judgment of Extent

Attempts to find a single anatomical or topographical source for the sensations which serve as criteria of extent of movement are contradictory and futile. Judgment and discrimination are inferential processes, founded in the empirically coordinated consequences of experience. Any sensitive part that is in any way uniformly stimu-

lated in the process of a given movement contributes its share to the character of the movement as a conscious fact, and any such contribution may be utilized in the recognition of the movement when it occurs again. A movement comes to be recognized as larger than others, not because it produces a more intense sensation, nor because of any geometrical correspondence of internal and external points, but because it has been *learned to be* a larger movement—one that will effect a greater change in an object with which we are dealing. Topographical treatment of the criteria of judgments of magnitude involves an "anatomist's fallacy."